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AND

RICHARD PHILLIPS, F.R.S. L. & E. F.G.S. &c.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Monit. Polit.* lib. i. cap. 1.

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PLATES.

ERRATA.

P. 165, l. 26, *for* cake *read* coke.
279, l. 5, *for* Bavaria *read* Baveno.
394, l. 18, *for* sulphurets *read* sulphates.
417, l. 21 from the bottom, and p. 418, l. 11, *for* Pelouse *read* Pelouze.
419, l. 19, *for* "the country," *read* "this country."
422, l. 19 from the bottom, *for* "as just described, *read* "as described in p. 418."

THE

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[THIRD SERIES.]

JANUARY 1837.

I. *Observations on the Crag, and on the Fallacies involved in the present System of Classification of Tertiary Deposits. By* Edward Charlesworth, *Esq., F.G.S., &c.**

I DID not venture to propose a separation of the marine deposits above the London Clay in Suffolk, until I considered myself in possession of something more than merely conjectural evidence to justify my division of these fossiliferous strata. That the immense accumulation of testaceous reliquiæ forming the Crag might in some places be seen to be separated from the subjacent beds of clay by a deposit the characters of which did not accord with the general aspect of either of these formations, was a statement involving mere personal observation, and which could therefore at any time be readily refuted or confirmed. But that this coralliferous stratum should be looked upon as holding an intermediate place not only in geological position, but in age, when considered in relation to the beds above and beneath it, was suggesting a notion which appeared to me so far admissible, that its adoption or rejection would entirely depend upon the results attending continued investigation. Anticipating the nature of the objections which I thought might probably be urged against my views, I endeavoured, in my first memoir†, to show that there were strong grounds for believing that the apparent agreement between the organic remains of the Coralline Crag

* Communicated by the Author.

† See Lond. and Edinb. Phil. Mag. for August 1835: vol. vii. p. 81.

and the superior beds, depended upon the abrasion or natural degradation of one deposit during the formation of the other. I then referred to the large proportion of Red Crag fossils which M. Deshayes had identified with species now known to inhabit the German Ocean (40 per cent.): consequently, if my idea of the removal of the fossils from an older to a more recent bed were disputed altogether, the number common to the two crag series would at any rate indicate no nearer approximation of the periods during which these *Testacea* existed than that established by M. Deshayes between those of the red crag and the *Mollusca* of our present seas. Under these circumstances, it was certainly with some degree of surprise that I found Mr. Lyell opposing the opinion I had advanced, upon no other ground than that of this very occurrence of analogous species in the two deposits*.

To say nothing of those Sicilian strata, which contain ninety-five per cent. of existing species, it is palpably evident that if a per centage of analogous forms, to the amount of thirty or forty, place in one and the same geological period the races of organized beings occurring in rocks naturally separated by superposition and mineral character, by the same line of reasoning must the red crag, in common with all the other pliocene deposits, be looked upon, geologically speaking, as part and parcel of the formations now going forward in the adjacent seas, although these very deposits have been referred to a distinct epoch by Mr. Lyell from the very circumstance of their containing 40 per cent. of existing species. Paradoxical as it may appear, the facts which in one instance are made use of to prove the wide interval which has elapsed between the deposition of certain marine strata, are on another occasion brought forward to establish diametrically opposite conditions. Thus a division called older pliocene is made for those beds which contain so few as 40 per cent. of species common to that period and the present, while the red and coralline crag must be identified,—because their fossils indicate just this same degree of approximation to one another. I apprehend that this is no other than a fair statement of the case, and that I have not pushed analogy beyond reasonable limits; for if we admit, with Mr. Lyell, that the formations of the present day constitute one link of the entire series, and originate in the continued operation of those causes which have been in activity, at least during the deposition of the supracretaceous rocks, we are surely justified in drawing analogous inferences, whether we are comparing the present deposits with the newer members of the tertiary series or the individual members constituting the tertiary group with one another.

* Lyell's Geology, 4th edit., vol. iv. p. 87.

Such then was the condition of the inquiry up to the time of a small series of shells from Ramsholt being placed by Mr. Lyell in the hands of M. Deshayes, and the result attending his examination of these fossils has been appealed to as one which must necessarily prove fatal to the views which I entertain as to the relative antiquity of the coralline crag *. A fellow-labourer in the field of geological research, presuming that all other sources of evidence must yield to the deductions arising from a concho-geological investigation, has been led, perhaps rather too precipitately, to exclaim, "If such be the fact, there is an end of the question between my opponent and myself." I trust, however, that without subjecting myself to the imputation of obstinately adhering to preconceived opinions, I shall be able to show that this question is not to be decided by quite so summary a proceeding. A critical examination into the real merits of the per-centage test, as a general rule for determining the comparative ages of tertiary deposits, will be found to exhibit such extensive limits of error in its practical application, that so far as the present inquiry is concerned, I am confident that no impartial observer would feel justified in endeavouring to form a conclusion, either on one side or the other, from the evidence which has yet been obtained from this source.

Before I proceed to discuss the value which should be attached to certain numerical calculations, I must briefly digress for the purpose of offering a few remarks upon the real nature of the discussion now pending, and its abstract geological importance.

There are doubtless some to whom it may appear a matter of little or no moment, whether we speak of these inferior beds as forming the lower part of the crag formation, so long as that geological distinction is made, or whether we consider them altogether as a distinct deposit. Here I would observe, in passing, that there is no *à priori* reason whatever why a distinct deposit should not be found between the beds called crag and the London clay. In fact, if we adopt Mr. Lyell's present classification of the British and Continental tertiary series, such a discovery would seem to be in every respect a desideratum, for the crag being placed in the pliocene period and the London clay in the eocene, a deposit of an intermediate age would lessen the hiatus between these two formations. Now among some of the important results which have arisen from the accumulated observations of geologists are certain general deductions, involving points of a physiological or general philosophical nature which possess an extreme degree of interest, apart from any connexion with geology as a

* See a paper by Mr. S. Woodward in Lond. and Edinb. Phil. Mag., vol. viii. p. 139; and also Mr. Lyell's address to the Geological Society, *Ibid.*, p. 327.

distinct science. Such are the changes of climate anterior to the historic æra, the comparative duration of species, and the wide geographical range which some extinct organisms enjoyed, having apparently existed at the same period over an immense extent of surface. These and numerous other inferences of a similar kind often depend in a great measure upon the accuracy with which we can refer individual beds or groups of strata to particular deposits, and trace these separate formations in widely remote localities, from observing the occurrence of certain corresponding phænomena. Philosophically speaking, it is, perhaps, of most importance in the fossiliferous rocks to make out the natural subdivisions (if such really exist) of the two extremes of the series; and in arriving at any generalizations analogically deduced from our present insight into the laws of nature, those deposits offer the most legitimate grounds for rational speculation which appear to have originated during an æra that impinges upon the present.

The most novel feature in the organic remains of the crag, considering the whole deposit as referrible to *one* period, is the occurrence of so many recent mammiferous species in one bed, and so large a series of extinct corals in the other. Now if the coralline crag be older than the stratum in which these mammalian remains are found, we have no longer this association of *extinct* forms in one class of the animal kingdom with *recent* types belonging to a very different order. A geologist desirous of instituting a comparison between these tertiary deposits and those in other parts of Europe, might meet with the equivalent of one only, assuming the red and coralline crag to be distinct. It would then become a question of great importance whether the organic remains included in the two series ought to be considered collectively or not, in endeavouring to establish an agreement with the fossils of supposed corresponding strata in distant localities.

Passing to generalizations of a different character, I will select one which I think illustrates in a particularly forcible manner the importance of allowing full play to the present investigation. The occurrence of some extinct mammiferous quadrupeds in deposits containing exclusively or nearly so recent *Mollusca*, has led Mr. Lyell to attribute a longer duration of species to the latter. Applying this argument to the fossils of the crag, we find conditions of another kind; for the forms which are most widely removed from existing types occur among the corals, while the majority of the mammiferous animals either closely *resemble* such as are now living, or can be identified with those which are imbedded in the alluvial or lacustrine deposits above the crag. Hence we might infer the short duration of the species of corals when compared with the *Mammalia*, and consequently the still shorter

period assigned to their existence if the comparison be drawn between them and *Mollusca*. In this way erroneous inferences with regard to the comparative duration of species, and other deductions of an equally important nature, might originate *solely* in an improper identification of the crag beds with one another.

I have made use of the above example as a means of showing how desirable it is that there should, if possible, be a right understanding as to the age of the coralline crag: but at the same time I would observe that Mr. Lyell's line of reasoning is one which should be applied with the utmost caution; for though it may be quite true that the remains of the mammoth have been found, as at Northcliff in Yorkshire, in conjunction with recent species of *Testacea*, yet before we can with justice found any argument upon the fact of their occurrence in the same deposit, it is absolutely requisite to show that association is a tolerably conclusive proof of contemporaneous existence: and having settled this point, (which is often no very easy matter, as will be seen in another part of this paper,) we must next inquire whether there be evidence of *anterior* coexistence during periods of equal duration. The fossil elephant of Yorkshire is found in the red crag, one of the older pliocene deposits; but the recent species of *Mollusca* with which in one case it was associated are not to be traced back to a period of corresponding antiquity. The duration which we are warranted in assigning to these latter is the time which has elapsed since the formation of the Yorkshire deposit, while we can date the existence of the elephant from the deposition of the red crag up to the period of its subsequent occurrence in the above-mentioned locality.

So far then as the progress of geology is concerned, I think ample reasons exist for prosecuting an inquiry into the relative ages of these tertiary beds; but the attainment of that object through the medium of numerical calculations involves the application of principles, the adaptation of which to the practical purposes of the geologist is an operation complicated in its nature, and which may also be often fallacious in its results.

Mr. Lyell's views upon this subject are so well known and have been so generally received, that without entering upon any detail respecting them, I may at once proceed to discuss the considerations which have led me to distrust the value of the per-centage test in those instances where we require something more than a general approximation towards accurate conclusions*.

* Some of the following facts and observations were drawn up as a continuation of a paper on this subject which appeared in the Supplement to the Phil. Mag. for June 1836. The delay in the publication has arisen from my wishing to lay them before the late Meeting of the British Association at Bristol.

When Professor Agassiz was on a late visit to this country, I was particularly anxious that that distinguished naturalist should have an opportunity of examining the ichthyological remains of the crag. With this view I endeavoured to obtain as extensive a series of these fossils as possible, and in the course of the summer of 1835 I collected several thousand bones, including vertebræ, teeth, and portions of palates, &c. A selection from these was submitted to the inspection of M. Agassiz just before he quitted England, and the result of his examination was, that among them he could detect no recent species, and that there were some belonging even to genera extremely remote from any with which he was acquainted. This was a result which I was not prepared to anticipate, as the crag had been classed by M. Deshayes among the pliocene deposits, in consequence of the large proportion of its shells which he had identified with recent species. On a subsequent occasion, however, when M. Agassiz had an opportunity of seeing my entire collection of crag fossils, after expressing great delight and astonishment at the novel structures exhibited by the corals, he mentioned to me his opinion that all the *Testacea* which he had seen from that formation were extinct. I cannot venture to say what amount of reliance should in this instance be placed on the opinion of Professor Agassiz, but certainly his zoological attainments are by no means confined to that particular department of scientific inquiry in which he has deservedly gained such extensive reputation. The observation thus casually made to me by him was shortly afterwards most unexpectedly confirmed by Dr. Beck of Copenhagen, who appears to have enjoyed very extensive facilities for the study of recent and fossil conchology. Dr. Beck communicated to me his opinion of the incorrectness of M. Deshayes' calculation before he had examined my collection, the inspection of which did not occasion any alteration in his views, as may be seen by Mr. Lyell's anniversary address*.

If we now turn to our own country we shall find a most remarkable discordance upon this subject in the opinions of British naturalists, although the balance is certainly not in favour of M. Deshayes. Mr. George B. Sowerby informs me that he has had many opportunities of comparing the crag shells with recent specimens, and that he has only found two or three shells which may perhaps be identified with living species.

In Professor Phillips's Guide to Geology, we find him placing the crag in the miocene division, probably estimating the proportion of extinct species at about seventy or eighty per cent. I need not here dwell upon Professor Phillips's general accuracy of observation and long familiar acquaintance

* See Lond. and Edinb. Phil. Mag., vol. viii. p. 327.

with organic remains. I may however mention that there is a large series of crag shells in the museum at York, from the examination of which I believe his opinion has been formed. In justice to M. Deshayes, I must now observe that there are several individuals to whose judgement I should be disposed to pay considerable deference, who think that in giving 40 per cent. he has considerably underrated the proportion of recent species, and that more than half, or perhaps three fourths of the crag shells can undoubtedly be identified with species now inhabiting the German Ocean.

The Rev. Dr. Fleming, in a letter to Dr. Mitchell, F.G.S., of London, in alluding to this subject, observes, "Many of the crag species are deep-water species, but I would fearlessly say they are of British origin, and I make the remark, having been an observer and collector of British shells for more than a quarter of a century."

In the annual address delivered by the President to the Fellows of the Geological Society, Mr. Lyell particularly adverts to the discordance of opinion between two such eminent naturalists as Dr. Beck and M. Deshayes, and suggests that it may probably be attributed to their difference of opinion as to the amount of variation necessary to constitute a distinct species. Thus, for instance, Dr. Beck would look upon those six or eight forms which M. Deshayes includes under the name of *Lucina divaricata* as six or eight distinct species of the genus Lucina, while M. Deshayes would consider them as varieties only. Now this explanation is only admissible upon the assumption that M. Deshayes allows the existence of as much difference between the crag fossils and what he now regards as their living analogues as there is between the six or eight varieties of the *Lucina divaricata*. This is an important consideration; for if M. Deshayes should assert the identification to be complete between the crag fossils and living shells, it is evident that the explanation offered affords no solution whatever of the difficulty.

From these facts the following inference may, I think, be fairly drawn: that if a series of tertiary fossils be placed before the most eminent conchologists in different countries, for the purpose of ascertaining from the per-centage of extinct species what position in a geological series the formation should hold from which these fossils have been obtained, that position might be decided to be, *eocene* in Denmark, *miocene* in England, and *pliocene* in France; and had we fifty intermediate gradations it is very possible that no two conchologists would refer the deposit in question to the same position.

Greatly as the discordance of these results is to be lamented, as retarding the progress of geology, it must mainly be attributed to the present imperfect condition of conchological sci-

ence, and not be supposed to invalidate the general course of induction pursued by Mr. Lyell. Nevertheless it must be admitted that the practical application of the principle advocated by this eminent geologist in the classification of the supracretaceous rocks will be extremely limited in operation; for even if we suppose that conchologists universally admit the soundness of the principles upon which the present system of chronological arrangement is founded, they cannot equally make use of it as a means of obtaining numerical relations of affinity, since the characters thought by one to constitute a distinction of species are by another looked upon as mere modifications of form.

Now, if we entirely throw aside all reference to a *per-centage* of species, and could substitute in its place a scale of degrees,—still taking the existing forms as a standard to which the fossil ones are to be referred, but determining the amount of approximation by the *totality* of the characters which each series exhibits,—we might then, perhaps, justly anticipate an agreement in the conclusions arrived at by different conchologists as to the relative age which should be assigned to any one fossiliferous deposit of the tertiary group; provided, of course, that there be no difference in their respective qualifications for conducting the necessary examination*.

Although Dr. Beck asserts that the *Testacea* of the crag have no existing analogues, thereby necessarily placing that formation in the *eocene* division of Mr. Lyell, yet as he admits a very considerable degree of resemblance in many of these fossils to species now living in the German Ocean, I apprehend that from that circumstance he would refer this deposit to a much more recent geological æra than the London clay. If I am right in this conjecture, it follows as a necessary consequence that there are two modes by which we estimate the degrees of affinity between fossils of separate deposits, or between fossil and recent species, one being the per-centage test, and the other that which must of necessity be employed by Dr. Beck were he to infer the greater antiquity of the organic remains of the London clay when compared with those of the crag.

Now it can be clearly proved that one of these modes is sometimes fallacious, as there are tertiary deposits to which if both tests be applied results completely at variance with each other will be evolved. Thus many of the forms occurring in the coralline crag are so unlike recent types, that if our estimate of its comparative age were taken from the totality of

* I am proceeding here upon the *supposition* that there is an uniform approximation to existing species, shown by the fossils of different deposits, corresponding to their respective antiquity.

the characters which its fossils, considered collectively, present, it would appear much older than the superjacent tertiary beds, but if the numerical test be made use of, the apparent age of both these deposits would be equal. Some remarks by Professor Phillips which have appeared in the *Encyclopædia Metropolitana*, and which were probably written before I had described the conditions under which the organic remains of the crag are deposited, bear very strongly upon the above statement. The passage is as follows: "Upon comparing them [that is, the crag fossils,] with recent kinds, we are presented with very curious and striking results. There are several of the crag shells so exceedingly similar to recent shells of the German Ocean that it is impossible to distinguish them. *Turbo littoreus* retains its colour, many others are with difficulty separated by minute discrimination; but some, as the *corals of Orford, Pecten Princeps, Terebratula Dalei, and others, are evidently unlike anything now existing in the German Ocean, and indeed not now to be paralleled in any part of the world.*"—Encyc. Metrop., Geology, p. 674.

Now the Corals, *Pecten* and *Terebratula*, spoken of by Professor Phillips as so utterly unlike anything now existing, are fossils of the *lower* or *coralline crag*. The *Turbo littoreus* retaining even its colour, so far as my own experience has gone, occurs only in that bed where we meet with existing species of Mammifera, and the more recent origin of which I have from the first endeavoured to establish.

From what I have advanced it will be seen that I am disposed to regard the source of error now under consideration as involved in the application of the per-centage test, and if the history of the crag be ever thoroughly worked out, I think this view will be confirmed. The fallacy probably consists in supposing that by number we can obtain a true expression of relations of affinity, when being totally ignorant of the characters which constitute species, we have really nothing upon which to found our numerical calculations.

I pass on from the consideration of this rather intricate question to another stage of the inquiry; and here for the sake of argument I must assume that there is a general agreement among conchologists as to the characters upon which specific distinctions are founded, and also that the true method of obtaining relations of affinity is the one which has been adopted by Mr. Lyell and M. Deshayes.

[To be continued.]

II. *Description of an Anchor found at Seaton, Devonshire.* *By Mr.* N. S. Heineken.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE anchor from which the accompanying drawings were taken, was found at Seaton, Devon, on the 25th of August 1836, nearly opposite to an opening on the coast called the Chan, and at a distance of about 500 fathoms from the shore. At this spot an obstacle had always occasioned damage to the seins of the fishermen, although for a considerable distance around this place the bottom was perfectly plain: many attempts had been made to ascertain the nature of the hinderance to fishing; but in vain until the time above stated, when ropes having been attached to the subject of this communication, it was, by the efforts of thirty or forty persons drawn ashore, very much, as may be supposed, to their amazement. I am told by my informant that the depth of the sea where it was found is about 15 fathoms, and that the oldest inhabitant does not remember that vessels have anchored within a quarter of a mile from the spot. From the peculiarity of its form the anchor would appear to be either of considerable antiquity, or of foreign make. The naval men who have inspected it, say, that the shank is from 16 to 18 inches longer in proportion to the arms than in anchors of the present time, and the shank also appears to have been square.

It is completely encrusted by a thick covering of sand, nearly approaching in hardness to sandstone, in which are imbedded rounded beach pebbles, shells of various kinds, *Serpulariæ*, *Balani*, &c.

Description of the Figures.

Fig. 1, is a front view of the anchor, showing at *a*, *b*, and *d*, masses of hard *marl*, by which it was attached to the ground.

Fig. 2, is a view of the other side, taken rather obliquely, in order to show the form of the fluke at *c*.

The dimensions are as follows:

Fig. 1, length of shank from *a* to *b*, 4 feet 6 inches.

Distance between the points of the flukes *c*, *d*, 2 feet 6 inches.

Thickness of marl at *a*	....	7½ inches.
Diameter ditto		8 inches.
Thickness of marl at *b*	....	7½ inches.
Diameter ditto		7 inches.
Width across fluke at *c*	....	8 inches.

Width across fluke at *d*, 7 inches.
Dimensions at *e*, $2\frac{1}{2}$ inches by $2\frac{1}{2}$ inches.
Ditto at *f*, 5 inches by 5 inches.
Weight of the whole from 150 to 200 lbs.

If the date of the anchor, from its unusual make, could be nearly ascertained, it would probably be interesting as indicating the period required for the formation of the incrustation (puddingstone?) around it in the peculiar situation in which it was found.

Fig. 1.

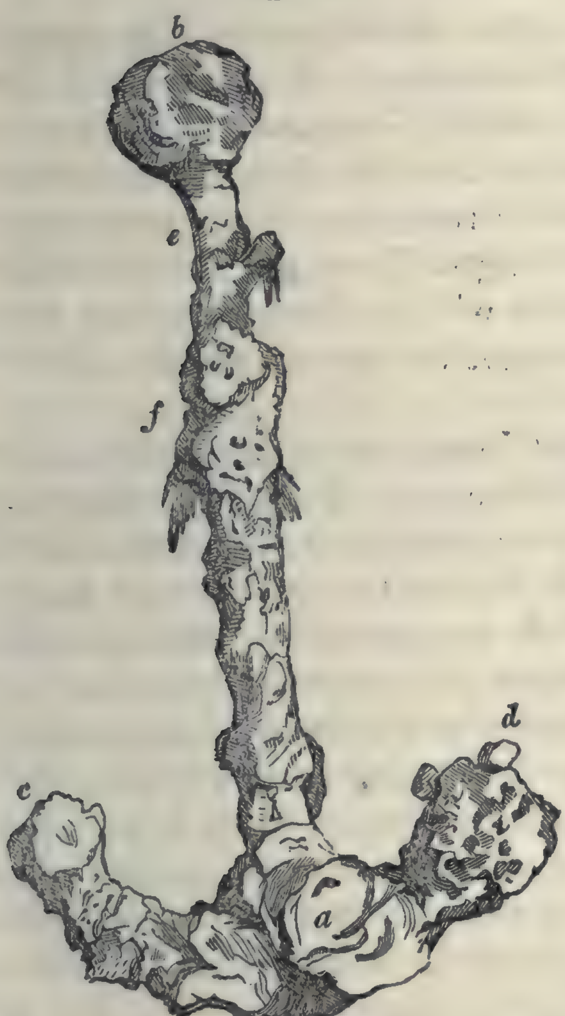


Fig. 2.

It may be remarked, that Leland observes that *formerly* Seaton was a place of considerable commercial importance, though in his time it was a mere fishing-town. It has also been stated that it had once three harbours. Some antiquaries have supposed it to have been the Moridunum of Antoninus, and in 937 it was the landing-place of the Danish princes. Now, whether the aforesaid anchor belonged to the Romans, or the Danes, or is of a more modern date, I leave to antiquaries to determine from its *make*, or geologists from its *coating*. I have done my endeavour to introduce it as a curiosity to their notice; and if *coveted*, it may, I doubt not,

adorn their museums, by proper application to its present owner, W. Champ, a fisherman residing at Seaton.

The sketches, I may observe, were taken with the camera ucida, and are therefore, I trust, tolerably faithful.

With respect, I am, yours, &c.

Sidmouth, Nov. 8, 1836. N. S. Heineken.

III. *On a Mode of obtaining increased Power from Magneto-electric Machines; in Reply to the* Rev. W. Ritchie, *LL.D.* *By* Fred. W. Mullins, *Esq., M.P., F.S.S., M.R.I., &c.**

I DID not receive the September and October Numbers of the Lond. and Edinb. Philosophical Magazine, till the commencement of the present month (November), and in consequence was not previously aware that Dr. Ritchie had made any observations in relation to my remarks on magneto-electric machines in page 120 of the August Number. I now hasten to correct some misapprehensions which it appears that gentleman has fallen into with respect to my statements in the paper alluded to.

Dr. Ritchie seems to think that I approve of bar magnets for magneto-electric machines in preference to horseshoe ones: in this respect he greatly mistakes me; for I distinctly stated that it was before trial I believed the former to be superior to the latter; but that "*after* trial" it appeared to me that the power of *all* such instruments was very imperfectly developed; which impression led to the inquiry and subsequent discovery already mentioned: so far from dissenting from Dr. Ritchie's opinion as to bar magnets, I fully concur with him in believing that of two bars of equal size tempered to the same colour, and magnetized equally, one of which is divided into two equal portions, and the other bent into the horseshoe form, the latter will exhibit the greater power: so far we agree; but if Dr. Ritchie means to assert that the power of the latter will exceed that of the former, *after the addition of the magnetized arcs*, then he must permit me to say, that I consider him to be decidedly in error, and feel myself, after further experiments, fully justified in adhering to my former opinion of the great superiority of bar magnets with magnetic arcs for magneto-electric instruments. I quite agree with Dr. Ritchie, that "facts are stubborn things to get rid of." Undoubtedly they are much to be preferred to arguments grounded on the imperfectly developed principles of a science

* Communicated by the Author.

as yet in its infancy; looking to facts therefore, I hope before long to have an opportunity of convincing Dr. Ritchie of the correctness of my results as already stated. I cannot well collect from *his* details of the second experiment, that Dr. Ritchie magnetized the connecting arc as well as the bars: for he says, "cut a bar into *three* portions; bend one into an arc, and magnetize the *two* parts." If he left, as I assume, the *third* part, or the arc, unmagnetized, it will be quite apparent that the *unmagnetic* arc will not give the power described by me: hence his error. The arc and the bars should be magnetized to saturation, and applied as I have before stated, namely, one to the opposing poles of the *front* bars, the other to those of the *back*; for if placed upon the intervening bars while the first-named are *unconnected*, the effect is not nearly so great.

Dr. Ritchie again mistakes me, when he understands me to assume, that connecting-pieces less than the dimensions given, would produce the same increase of power; my remarks were made in relation to the size of the arcs described, as compared with *larger* ones, and went to show that there was a limit to the increase of power, in as much as augmented power did not accompany enlargement of the arcs, and this is the fact; but it does not at all follow, that because this is the case, smaller arcs than those described would prove as efficacious; for it is easy to conceive that the arcs should contain a certain amount of aggregated particles *magnetically* combined with the electric element, in order to produce the maximum increase of power in the bars; but that *beyond* that amount no increase of the particles could augment the power obtained, without a proportional augmentation of the bars, or more strictly of the magneto-electrized particles of the bars.

I am quite free to acknowledge that I did not express myself upon this point in terms as precise as I could wish, but a little consideration would, I am certain, have shown Dr. Ritchie that I merely meant to signify *that size beyond a certain limit previously defined, did not augment the power produced.*

As I mean to write more in detail upon these matters as soon as I shall have obtained the results of a course of experiments in which I have been for some time engaged, I will not go further into this interesting subject at present.

Beaufort-House, Nov. 4, 1836. FRED. W. MULLINS.

IV. *Additional Remarks on* Mr. Hopkins's "*Researches in Physical Geology.*" *By* Henry S. Boase, *M.D., &c., Secretary to the Royal Geological Society of Cornwall.**

IT was with great pleasure that I read Mr. Hopkins's reply to my former remarks, because a discussion conducted in a proper spirit cannot fail to elicit truth, and may be the means of stimulating geologists to investigate more carefully the important question under consideration.

The point at issue between us is, as Mr. Hopkins has justly remarked, "whether the jointed structure of disturbed masses has been in great measure superinduced *previously* or *subsequently* to their elevation." Two other topics have been dwelt on in his reply,—the nature of the elevatory force, and the origin of veins,—both very interesting, but not, I conceive, so easily determined by *observation*, as the question more immediately under discussion.

Mr. Hopkins asserts, "that it is totally inadmissible to assume the earth's crust to have become jointed, before the action of the dislocating force upon it." I, on the other hand, contend that *solid* rocks not only existed *previously* to their having experienced elevatory movements, but also that such rocks must necessarily have had a *jointed structure*. This is, I think, a fair and plain statement of the case *sub judice*, divested of all its collateral intricacies: now then for the evidence.

As regards the structure of rocks, how does the matter stand at the present day? If we examine any formation from the oldest non-fossiliferous strata to the newest of the tertiary deposits, or indeed to the recent sandstones of the modern or diluvial epoch, the evidence is invariably the same: all demonstrate that *solid* rocks, whether they have or have not been subject to movements, possess a concretional structure, being intersected by *lines or joints* which divide them into determinate masses. And not only so, but granitic and trappean rocks, and even lavas, all, when solidified, are similarly circumstanced. Nor can this excite any wonder, since a contrary state of things would not be in accordance with the laws of nature,—it being a fundamental maxim in physics, that the particles of solids are united together by attraction of cohesion, which has a tendency to arrange bodies in definite forms. If we did not know the fact, would it not have been a legitimate inference that solid mineral masses might be found to possess a concretional structure? But in as much as all known *solid* rocks, whether igneous or aqueous, disturbed or

* Communicated by the Author.

in their original position, in mass or insulated between loose earthy beds,—in as much, I say, as all these exhibit joints or lines of structure, I think that I am justified in regarding this condition as a species of crystallization,—the inseparable consequence of the particles of an originally unconsolidated mass having been completely subjected to the operation of cohesive attraction.

If Mr. Hopkins's hypothesis requires it to be otherwise, it is incumbent on him to adduce facts in support of his opinion. Perhaps, he will not dispute this position, but content himself with maintaining that the rocks, when elevated, were not in a solid state. I shall, however, have no difficulty in establishing the contrary; for there is ample evidence of rocks, before dislocation and elevation, having been solidified.

But, says Mr. Hopkins, "in my investigation it is unnecessary to suppose any but the lowest degree of solidification in the elevated mass; and therefore it is manifestly quite inadmissible to assume that it could not be dislocated by an elevatory force before its jointed structure had become sufficiently developed to determine the directions of dislocation." I do not *assume* that an unconsolidated mass cannot have been elevated or depressed; because it is evident, that recent sedimentary deposits must be acted on, according to the movements of the older *solid* rocks on which they repose; but I do assert that rocks over extensive regions in every part of the globe, (and only one instance would suffice for the argument,) when thus acted on, were not in the lowest, nor in any intermediate degree of the process of induration, but were perfectly consolidated.

For instance, various series of rocks, including those of more than one epoch, have accumulated, during the lapse of ages, from the comminuted debris, angular and water-worn fragments of older rocks, in the hollows of which they have been deposited; these derivative rocks now exhibit faults, veins, and other indications which Mr. Hopkins ascribes to elevatory movements. Now it is of no consequence whether these upper derivative rocks were solid or not; but it is evident that the parent fundamental rocks must have been perfectly *solid* or they could not have furnished the pebbles, which very commonly consist of quartzose and other siliceous substances, not only belonging to older sedimentary formations, but also to *igneous* rocks which cannot be supposed to have been reduced to a state of detritus by aqueous action until they were actually *solid*. It may also be remarked, that it is generally admitted that movements, such as have taken place in former days, are now and will be hereafter in operation:

there can be no doubt then that all future dislocations must affect a basis of *solid* rocks possessing lines of structure, and therefore would be subject to the modifying circumstances which Mr. Hopkins is desirous of evading.

In opposition to this statement, can Mr. Hopkins adduce any evidence in support of his conjecture, that various degrees of solidification existed in all the rocks subjected to elevatory movements? If not, the convenience alone of his hypothesis ought not to be admitted as a sufficient argument; and my deduction from facts cannot be considered as satisfactorily answered by treating it as "*à priori* reasoning founded on what we are altogether ignorant of."

In order to illustrate the subject more fully, let us direct our attention to the principal movements which Cornwall is supposed to have undergone. These appear to be referrible to four periods, marked by—

1st. The protrusion of the granite through the stratified rocks, tilting them up at various angles, and injecting granite in the form of veins into the adjacent fissures.

2nd. The formation of porphyritic dykes or *elvan-courses* which traverse both the granite and the slate.

3rd. The production of metalliferous veins, intersecting the granite, slate, and elvans; and,

4th. The introduction of another system of veins, traversing all the preceding formations, locally termed *cross-courses*.

1. What was the condition of the stratified rocks, when the first and most remote movement occurred? Could it have been at the lowest degree of solidification, or indeed, at any degree short of absolute solidity? I think not. 1st, Because, admitting that process to have been "the gradual work of lengthened periods of time," we have a very sufficient limit in the countless ages which must have elapsed between the deposition of the non-fossiliferous strata of Cornwall and the formation of the carboniferous or the saliferous group, whichever may be determined to mark the period when the granite was protruded. 2ndly, Because the nature of the detritus derived from the older strata, and contained in the conglomerates formed before the elevation of the granite, indicate that the parent rocks must have been in a *solid* state. And lastly, because the sharp angular portions of slate, included within the granite veins, demonstrate that they could not have been forcibly detached from rocks only partially *solid*. Thus then we see that even *in limine* Mr. Hopkins has great difficulties to contend with, in refusing to admit the perfect solidity of the inferior disturbed strata; and these must necessarily be increased at each successive step. But before advancing

we must allow the melted granite to cool, and the supposed belt of altered slate to assume its new or superinduced lines of structure. This being accomplished, then follows the—

2nd. Movement, as denoted by the formation of fissures, in which the dykes of elvan now occur, and which run continuously through both granite and slate. We have seen that the latter rock was already solid, and there can be little doubt that the granite was also in a similar state; for very highly inclined fissures, twenty to fifty feet in width, could scarcely have been maintained in an ignited mass, possessed of any degree of viscidity; because, if in any state short of solidity, the incumbent pressure would cause the mobile mass to sink into and obliterate such fissures.

3. The movement which gave origin to the metalliferous veins seems also to have operated on *solid* rocks, since they cut through granite, slate, and elvan which must have been previously in a state to furnish considerable quantities of angular portions of all these rocks with which the veins abound: independently of the fact that this movement was subsequent to former dislocations which, it has been shown, occurred in rocks already solidified.

4. The movement, indicated by the *cross-courses* also containing detached portions of rocks, must likewise have been effected in a solid mass. It is superfluous to make any further remark on this head; but I will here observe, that Mr. Hopkins has fallen into an error in stating that *cross-courses* are universally recognised to be of irregular width, as compared with the "bearing" veins. The fact is, that both systems are exceedingly irregular in this respect; but if any rule obtains, it is the reverse of Mr. Hopkins's statement. Some importance appears to have been attached to this difference in width, but I cannot detect the nature of its bearing.

From these facts* it appears, that in Cornwall the rocks

* In discussing this subject I have endeavoured to keep the argument as simple as possible, and therefore have not dwelt on the phænomena of veins. But as a specimen how the difficulty of Mr. Hopkins's position increases when applied to some only of the details, I may observe that two parallel systems of veins frequently occur inclined towards, and intersecting each other, at great angles; whilst they are both traversed by a third system or cross-courses. All these veins are parallel to, and partially identical with three systems of joints or lines of structure, dividing the mass into concretions which are generally of a rhomboidal form. Here, then, (without complicating the matter still further with joints and veins, which in Cornwall, and probably in other countries, traverse the quadrilateral concretions diagonally,) we have systems of veins crossing each other at acute angles, a condition which Mr. Hopkins has stated cannot have been produced by the elevatory or other extraneous forces, as these, he says, "could only tend to produce systems of fissures crossing each other at right angles."

were *solid previously* to their elevatory movements; and that they also possessed lines of structure is more than probable, since the fragments contained in the granite veins, in the elvans, and in the two systems of mineral veins, exhibit the *same* concretional forms as those into which the corresponding rocks are now divided by weathering or mechanical action.

Thus I have endeavoured to substantiate my former statement, that the elevatory force could not have acted on a *solid* mass without the interference of *lines of structure*; a circumstance which would produce, according to Mr. Hopkins, such considerable modifications in the resulting phænomena, that "to a mass thus constituted these [his physical] investigations must not be considered as generally applicable."

When I commenced this reply, it was my intention to have offered a few remarks on Mr. Hopkins's lengthened comments on the hypothesis which I have advocated concerning the origin of mineral veins in *primary* districts; but as it is immaterial, in the present case, whether the veins, granite, and slate are or are not all contemporaneous, I think it best not to have our attention diverted from the point at issue, which must be determined by facts, and not by the gratuitous *postulata* of either hypothesis.

I cannot, however, conclude without again acknowledging the great obligation geologists are under to Mr. Hopkins for his interesting investigations; and, though differing from him on some points, I am not insensible to the great advantages which must accrue to geology in controlling wild speculations by the application of the rigid laws of physical science.

Penzance, Nov. 8, 1836.

V. *On the true and extended Interpretation of Formulæ in Spherical Trigonometry.* By James Thomson, *LL.D., Professor of Mathematics in the University of Glasgow.**

1. WHILE the rules and theorems given in the modern books on trigonometry, in reference to spherical triangles, are sufficient for all practical purposes, yet there are some peculiarities and some curious relations of such triangles, which have either been overlooked in all the works with which I am acquainted, or have been merely glanced at in casual or passing remarks; and hence, as may be expected, some parts of the theory are still imperfectly developed. I shall proceed†

* Communicated by the Author.

† The formulæ quoted in this paper will be found in my Elements of Plane and Spherical Trigonometry, and in most of the modern treatises on the subject.

to give some instances of this kind, and shall commence with the formula

$$\cos A = \frac{\cos a - \cos b \cos c}{\sin b \sin c},$$

which enables us to solve the problem in which the three sides are given, to find the angles. Now, since, if Q be any arc or angle, $\cos Q = \cos (2\pi - Q)$, it is plain, that if A' denote one value of the angle A, there will be another value $2\pi - A'$, unless it can be shown that this value is excluded for some reason not indicated by the foregoing formula. Let the former of these be less than two right angles; then, if the latter, which we may denote by A'', be admissible, it will be greater than two right angles*. In like manner the corresponding formulæ would give two expressions, B' and B'' $(= 2\pi - B')$, for B; and two, C' and C'' $(= 2\pi - C')$, for C. Now the first of these, A', B', C', regarded as being each less than two right angles, are those which are universally, and, in a practical point of view, correctly employed. On consideration, however, we shall find that the latter values are just as admissible as the former. When a great circle is described through two points on the surface of a sphere which are not the extremities of a diameter, the points may be regarded as being *joined* by either of the arcs into which they divide the circle: it is usual, however, to consider the *smaller* arc as the one which joins them. Now, if we take on a spherical surface three points which are not on the same great circle, and join them in the way last mentioned, by three arcs, a, b, c, we shall obviously divide the entire surface into two parts, each bounded by three arcs of three great circles, and therefore each of them a spherical triangle. The smaller of these is that which is usually alone considered, and its angles are A', B', C'. The greater has for angles A'', B'', C''; and, having a, b, c as sides, it answers the conditions of the problem just as well as the smaller triangle. The angles of the greater triangle are evidently each greater than two right angles.

* It is scarcely necessary to state, that there is no impropriety in regarding an angle as greater than two right angles. If we suppose one radius of a circle to be fixed, and another to revolve from a state of coincidence with it, the motion of the latter will make continual additions to the quantity of angular space described; and the angle made by the lines, commencing from nothing, may be regarded as increasing so as to obtain any magnitude we please. If the line revolve in one direction, the angle may be regarded as positive; if in the other, negative. It is plain also, that if any angle Q has been described, the relative positions of the lines will be the same again after the description, once or oftener, of four right angles in either direction; or, as it may be expressed, the relative positions of the lines will be the same, when the angle which they make is Q, as when it $Q \pm 2n\pi$, n being a whole number.

2. What has been thus established affords an explanation of the double sign $\pm$, before the expressions for the sine and cosine of half any angle of a spherical triangle. Thus, in the formula

$$\sin \tfrac{1}{2} \mathrm{A} = \pm \sqrt{\frac{\sin (s-b) \sin (s-c)}{\sin b \sin c}},$$

if we denote the value of $\frac{1}{2}$ A corresponding to the sign +, by $\frac{1}{2}$ A′, we shall have the other, corresponding to −, equa to $-\frac{1}{2}$ A′; whence the values of A are A′ and −A′; or, by adding 2π to the latter, according to the preceding note, the values will be A′ and $2\pi-$A′. We see, therefore, that the positive value of $\sin \frac{1}{2}$ A gives the value of $\frac{1}{2}$ A in the less of the two triangles bounded by a, b, c; and the negative value that of $\frac{1}{2}$ A in the greater*.

3. In like manner, in the formula

$$\cos \tfrac{1}{2} \mathrm{A} = \pm \sqrt{\frac{\sin s \sin (s-a)}{\sin b \sin c}},$$

the positive and negative values will give respectively $\frac{1}{2}$ A equal to $\frac{1}{2}$ A′ and $\pi-\frac{1}{2}$ A′; and consequently A equal to A′ and $2\pi-$A′, as before: and, in a similar manner, we may explain the double sign in the formulæ

$$\tan \tfrac{1}{2} \mathrm{A} = \pm \sqrt{\frac{\sin (s-b) \sin (s-c)}{\sin s \sin (s-a)}},$$

and $$\sin \mathrm{A} = \pm \frac{2 \sqrt{\sin s \sin (s-a) \sin (s-b) \sin (s-c)}}{\sin b \sin c}.$$

4. By considering in a similar manner any of the formulæ which determine the sides by means of the angles, we shall arrive at other results which do not seem to have been hitherto observed. Thus, from the formula

$$\cos a = \frac{\cos \mathrm{A} + \cos \mathrm{B} \cos \mathrm{C}}{\sin \mathrm{B} \sin \mathrm{C}},$$

and the corresponding ones for $\cos b$ and $\cos c$, we see that when the three angles are given, each of the sides has two values of the forms a' and $2\pi-a'$, b' and $2\pi-b'$, c' and $2\pi-c'$. The values a', b', c', each less than a semicircle,

* The same conclusions might be derived from the formula for $\sin \frac{1}{2}$ A, taken, not with both its signs, but with either. For, since $\sin \mathrm{Q} = \sin (\pi-\mathrm{Q})$, we should have for the value of $\frac{1}{2}$ A, corresponding to the positive sign, either $\frac{1}{2}$ A′ or $\pi-\frac{1}{2}$ A′; and consequently A equal to A′ or $2\pi-$A′, as before; while the negative value will give $-\frac{1}{2}$A′ and $-\pi+\frac{1}{2}$A′; by doubling which, and adding to each 2π, we get $2\pi-$A′ and A′, as before. The same may also be done by means of the formula for $\cos \frac{1}{2}$ A.

are those which alone are recognised by writers on trigonometry. With regard to the others, they are evidently the remaining arcs of the great circles of which a', b', c' are parts, and they are therefore each greater than a semicircle. In fact, if the triangles be described, whose sides are a', b', c', and whose angular points are A, B, C, so that A and B are joined by c', A and C by b', and B and C by a'; then $2\pi - c'$ is the *other arc* joining A and B; $2\pi - b'$ the other arc joining A and C, &c. These larger arcs, being the continuations of a', b', c', will evidently form with each other angles which are vertically opposite, and therefore equal, to those made by a', b', c'. They will therefore answer to the conditions of the problem, since they are arcs of great circles making with each other angles equal to the given angles.

5. It ought to be remarked that a'', b'', c'' do not form on the surface of the sphere, a triangle in the ordinary sense of the term: *they do not form a space which they bound without intersecting it.* The truth is, however, that in both plane and spherical trigonometry, so far as the computation of sides and angles alone is concerned, we have nothing to do with the *surface* of the triangle, or with any surface whatever; *the lengths of lines and their relative positions* being the sole subject of consideration. Thus, when the three sides of a spherical triangle are given, to find the angles, the problem is simply this: *Given three points on the surface of a sphere, to find the angles made by the great circles passing through them.* So likewise, when the three angles of a spherical triangle are given, to find the sides, the problem, without reference to a triangle, is simply, *to find three points on the surface of the sphere, such that the arcs joining them may make with each other angles equal to given angles*: and it is easy to see that other problems may be expressed in a similar way; and that the same views, as well as some others in this paper, may also be extended to plane triangles.

6. The problem in which two sides of a spherical triangle, and the contained angle are given, to compute the remaining angles and the third side, may also be viewed in a similar light. Thus, the third side is found by means of the formula

$$\cos a = \cos A \sin b \sin c + \cos b \cos c;$$

and it is plain, that a will have two values of the forms a' and $(a'' =)\, 2\pi - a'$. The smaller is that which is recognised by the writers on trigonometry as the third side; while the other is the remaining arc of the great circle of which a' is one part. This will be illustrated by taking on a common globe a point

above the horizon, and drawing from it two arcs, b and c, of great circles to the horizon. Then, if we regard these two, and the angle which they form, as given, we shall have one triangle, which will answer the conditions of the problem, bounded by b and c, and the less arc of the horizon joining their extremities; while with the greater or remaining arc of the horizon they will form another triangle, which will answer equally. The latter triangle is evidently composed of the former, together with the hemisphere below the horizon.

7. The remaining angles will be found by the formulæ

$$\cot B = \frac{\cot b \sin c - \cos A \cos c}{\sin A}$$

and

$$\cot C = \frac{\cot c \sin b - \cos A \cos b}{\sin A}.$$

From these, since $\cot Q = \cot(\pi + Q)$, we shall have values for B and C of the forms B', C', and $\pi + B'$, $\pi + C'$, which evidently correspond to the triangles above mentioned.

8. When a side a, and the adjacent angles B and C, are given, the remaining sides may be found by means of the formulæ

$$\cot b = \frac{\cot B \sin C + \cos C \cos a}{\sin a}$$

and

$$\cot c = \frac{\cot C \sin B + \cos B \cos a}{\sin a}.$$

These give values for b and c, of the forms b', c', and $\pi + b'$, $\pi + c'$; the first of which are the values adopted in the books on trigonometry. The meaning of the others will appear if we enunciate the problem thus: Two great circles being drawn through B and C, the extremities of a given arc a of a great circle, and making with it given angles; it is required to find the distances between the points B and C and the two points of intersection of these circles. These circles will intersect in a point A; and if they be continued through that point each to a distance equal to π, they will again intersect in a point A''. C A and B A are evidently the arcs b' and c'; while C A A'' and B A A'' are respectively $\pi + b'$ and $\pi + c'$. The third angle comes out, as it ought, of the forms A' and $2\pi - A'$, the former being the angle at A, and the other the one at A''.

9. The case in which there are given a side and the opposite angle, and either another side or another angle, to resolve the triangle, may be treated in a similar manner; but neither case presents any difficulty, and they exhibit nothing remark-

able in addition to what has been pointed out in the other cases.

10. I shall conclude with some remarks which naturally arise from what has been already said.

(1.) Dr. Simson and others define a spherical triangle as being " a figure upon the surface of a sphere comprehended by three arcs of three great circles, *each of which is less than a semicircle.*" The words in italics should be omitted. Analysis, as we have seen above, detects the error, and shows that a side may be of any magnitude not exceeding a complete circle. An author, as the writers on trigonometry have done, may properly confine his attention to triangles having each of their sides less than a semicircle, but he ought not to state that there can be no others.

(2.) The proposition is not true, in which it is asserted, that " the three sides of a spherical triangle are together less than a circle." For a triangle understood in the ordinary meaning, (as bounded by three arcs of great circles, without being intersected by them,) the limit is *two circles*, instead of one. To illustrate this familiarly, let us take two points, A and B, on the horizon of a globe, very near each other, and a third C, nearly diametrically opposite to them, but above the horizon; then draw the smaller of the arcs joining A C and B C. By these two arcs, and by the greater arc of the horizon, the sphere will be divided into two triangles, each having one side almost a circle, and each of the others nearly a semicircle.

(3.) Neither is the proposition true, in which it is asserted, that " any two sides of a spherical triangle taken together, are greater than the third side." It is easy to see, by taking one of the sides almost a circle, and the others small, that one side may exceed the sum of the other two in any ratio whatever.

(4.) The assertion is likewise erroneous, that " the sum of the three angles of a spherical triangle cannot be less than two right angles, nor greater than six." The true limits are *two right angles and ten right angles.* To illustrate this, let the surface of a sphere be divided into two triangles having extremely small sides. Then, the angles of the smaller triangle being A, B, C, those of the other will be $2\pi - A$, $2\pi - B$, and $2\pi - C$. The sum of the former will be $\pi + E$, where E may be as small as we please; while, by adding the others, we find for their sum $6\pi - (A + B + C)$, or $6\pi - \pi - E$, or finally, $5\pi - E$; that is, ten right angles wanting E.

(5.) The area of a spherical triangle may be of any magnitude between zero and the surface of the sphere.

(6.) The same principles explain some things regarding the

spherical excess, which seem to have been hitherto overlooked. Thus, E being the spherical excess, the known formula

$$\sin \tfrac{1}{2} E = \pm \frac{\sqrt{\sin s \sin (s-a) \sin (s-b) \sin (s-c)}}{2 \cos \frac{1}{2} a \cos \frac{1}{2} b \cos \frac{1}{2} c}$$

gives for $\frac{1}{2}$ E, on account of the double sign $\pm$, or of the two values belonging to $\sin \frac{1}{2}$ E, or $-\sin \frac{1}{2}$ E, two values, of the form $\frac{1}{2} E'$ and $2\pi - \frac{1}{2} E'$, and, consequently, for E the two E' and $4\pi - E'$. Now, the less of these, suppose E', answers to the smaller of the two triangles formed by a, b, c, and the other to the greater. For the excess in the former is $A+B+C-\pi$; while in the latter, it is $5\pi-(A+B+C)$, or $5\pi-(\pi+E')$, or, finally, $4\pi-E'$.

(7.) In like manner, the formula

$$\cot \tfrac{1}{2} E = \pm \frac{1+\cos a+\cos b+\cos c}{2\sqrt{\sin s \sin (s-a) \sin (s-b) \sin (s-c)}},$$

gives for $\frac{1}{2}$ E, on account of the double sign, values of the form $\frac{1}{2} E'$, and $2\pi - \frac{1}{2} E'$. So likewise, from Lhuillier's formula

$$\tan \tfrac{1}{4} E = \pm \sqrt{\tan \tfrac{1}{2} s \tan \tfrac{1}{2} (s-a) \tan \tfrac{1}{2} (s-b) \tan \tfrac{1}{2} (s-c)},$$

taking the value of $\tan \frac{1}{4}$ E first positive and then negative, we find for $\frac{1}{4}$ E values of the form $\frac{1}{4} E'$, and $\pi - \frac{1}{4} E'$; whence E will be of the forms E' and $4\pi - E'$, as before.

(8.) From the formula $\cot \frac{1}{2} E = \dfrac{\cot \frac{1}{2} a \cot \frac{1}{2} b + \cos C}{\sin C}$,

which gives the excess when two sides and the contained angle are the data, we find $\frac{1}{2}$ E to be of the forms $\frac{1}{2} E'$ and $\pi + \frac{1}{2} E'$; whence the forms of E will be E' and $2\pi + E'$. This answers exactly to the two triangles mentioned in No. 6; since the excess E' in the smaller is $A+B+C-\pi$, and in the larger $A+\pi+B+\pi+C-\pi$, or, by contraction, $2\pi+A+B+C-\pi$, which is the same as $2\pi+E'$.

Glasgow College, Oct. 21, 1836.

VI. *Demonstrations of certain points in* Fresnel's *Theory of Double Refraction, deduced from the Investigations of the Undulatory Theory which have recently appeared in this Journal.* *By* A CORRESPONDENT.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I DO not know whether the following results are sufficiently original to authorize an expectation that you will be able to afford them a place in your Journal, as, at the most, they

are but demonstrations, in perhaps a somewhat new form, of one or two points of Fresnel's Theory of Double Refractions. Such as they are, however, I submit them to your notice, as obvious and satisfactory deductions from the valuable papers on the undulatory theory which have recently appeared in your periodical.

On reference to Mr. Tovey's communication in the January number, 1836, (vol. viii. p. 8,) it will be seen that

If m, m_1, m_2, &c. be the molecules of an elastic medium,

x, y, z, the coordinates of rest of m,

$x + h_{\prime}, y + k_{\prime}, z + l_{\prime}$ ...... of $m_{\prime}$,

&c. ....

$\gamma_{\prime} = \sqrt{h_{\prime}^2 + k_{\prime}^2 + l_{\prime}^2}$, &c. = &c.,

and if, the system being disturbed, the displacements of m at the time t be u, v, w || the coordinate axes, those of $m_{\prime}$, $u + \delta_{\prime} u$, $v + \delta_{\prime} v$, $w + \delta_{\prime} w$, &c., then the accelerating forces on m in consequence of the disturbance will be

$$\xi\, m\, \{\phi\, r\, .\, \delta u + (h\, \delta u + k\, \delta v + l\, \delta w)\, h\, \psi\, r\} \qquad || \, x,$$
$$\xi\, m\, \{\phi\, r\, .\, \delta v + (h\, \delta u + k\, \delta v + l\, \delta w)\, k\, \psi\, r\} \qquad || \, y,$$
$$\xi\, m\, \{\phi\, r\, .\, \delta w + (h\, \delta u + k\, \delta v + l\, \delta w)\, l\, \psi\, r\} \qquad || \, z,$$

where $\phi\, r = \frac{f\, r}{r}$, $\psi\, r = \frac{f'\, r}{r^2} - \frac{f\, r}{r^3}$, the action of a molecule m on another at the distance r from it in the direction of their distance being $m\, f\, r$, and the sign ξ extending to all the molecules within the sphere of m's action.

Now at the beginning of the motion we have

$$\delta_{\prime} u = \delta_2 u = \text{\&c.} = -u,$$
$$\delta_{\prime} v = \delta_2 v = \text{\&c.} = -v,$$
$$\delta_{\prime} w = \delta_2 w = \text{\&c.} = -w,$$

m_1, m_2, &c. not having yet been disturbed; and ∴

$$\left.\begin{array}{l} \text{initial force} \; || \, x = A\, u + H\, v + G\, w = \rho \\ \qquad\qquad \{A \cos\alpha + H \cos\beta + G \cos\gamma\}, \\ \text{———} \; || \, y = H\, u + B\, v + F\, w = \rho \\ \qquad\qquad \{A \cos\alpha + B \cos\beta + F \cos\gamma\}, \\ \text{———} \; || \, z = G\, u + F\, v + C\, w = \rho \\ \qquad\qquad \{G \cos\alpha + F \cos\beta + C \cos\gamma\}, \end{array}\right\} \text{(A.)}$$

where ρ is the whole displacement of the molecule, making ∠s α, β, γ with the axes, and

$$A = -\xi m(\phi r + h^2 \psi r), \qquad F = -\xi m k l \psi r,$$
$$B = -\xi m(\phi r + k^2 \psi r), \qquad G = -\xi m h l \psi r,$$
$$C = -\xi m(\phi r + l^2 \psi r), \qquad H = -\xi m h k \psi r,$$

being quantities depending only on the nature of the medium, and not at all on the quantities ρ, α, β, γ, though in the same medium they may vary with the position of m. Hence if R be the whole force on m, developed by its displacement ρ, and λ, μ, ν the $\angle$s its direction makes with the axes, we have $R = \rho k$, where

$$\begin{aligned} K^2 = {} & (A^2 + G^2 + H^2)\cos^2\alpha + (B^2 + F^2 + H^2)\cos^2\beta, \\ & + (C^2 + F^2 + G^2)\cos^2\gamma + 2H(A+B)\cos\alpha\cos\beta, \\ & + 2G(A+C)\cos\alpha\cos\gamma + 2F(B+C)\cos\beta\cos\gamma, \end{aligned}$$

and
$$\begin{aligned} K\cos\lambda &= A\cos\alpha + H\cos\beta + G\cos\gamma, \\ K\cos\mu &= H\cos\alpha + B\cos\beta + F\cos\gamma, \\ K\cos\nu &= G\cos\alpha + F\cos\beta + C\cos\gamma. \end{aligned}$$

Generally, therefore, the whole force on $m \propto$ displacement, but does not act in the direction of the displacement. Moreover, from the linear form of the equations (A) it appears that we must obtain the same values for the component initial forces on m, and therefore the same whole initial force, both in magnitude and direction, whether we suppose the whole displacement ρ communicated at once to m, or the component displacements u, v, w separately communicated, and take the sums of the separate forces which would be thus produced.

Suppose now there be a direction of displacement, such that the whole force developed on m acts in the direction of the displacement, *i.e.* that $\lambda = \alpha$, $\mu = \beta$, $\nu = \gamma$, then our three preceding equations assume the well-known form of the equations which occur in the investigation of the principal axes of a body, and the cubic resulting from the elimination of $\cos\lambda$, $\cos\mu$, $\cos\nu$, will be

$$\begin{aligned} & (K-A)(K-B)(K-C) - F^2(K-A) - G^2(K-B) \\ & \quad - H^2(K-C) = 2FGH. \end{aligned}$$

The results, therefore, will be similar in the two cases, *i.e.* there will always be three, and generally only three, such directions.

Suppose we have determined one of these, take it for axis of z; then, since a displacement in direction of this axis produces a force also in that direction, it follows from (A) that $F = G = 0$, and that for this axis $K = C$. Suppose also that the plane of $x z$ passes through another of the axes, then to determine its position we have the equations

$$A \cos \alpha = K \cos \alpha,$$

$$H \cos \alpha = 0,$$

$$C \cos \gamma = K \cos \gamma.$$

Hence if K does not again $= C$, we have $\gamma = \frac{\pi}{2}$, and $\therefore$ $\alpha = 0$, $H = 0$, and K for this axis $= A$; if, however, K again $= C$, which cannot happen, as appears from considering the first of these equations, unless $A = C$ γ, and therefore α remains indeterminate, H vanishes, and $K = C = A$. If, then, A and C are unequal, this second axis is $\perp r$ the first; but if $A = C$, any axis in a certain plane passing through the first possesses the required property; and in either case the value of K for this axis is A; similarly with respect to B and C. Giving then the name of axes of elasticity to a set of three rectangular axes, through a molecule possessing the property under consideration, it follows (since the same inferences which have resulted from supposing the axis of z determined as above would have been deduced from the same supposition with respect to the axes of x and y,) that there is at least one set of such axes through every molecule of a medium. Take these then as axes of coordinates; then if A, B, C are unequal there can be but one set; if any two of A, B, C, *e.g.* A, B, are equal, there are an infinite number of sets, having one axis, that of z, common to each; if A, B, C are all equal, any set of rectangular axes through m are axes of elasticity.

Retaining the axes of elasticity as coordinate axes, we have $F = G = H = 0$; and the initial forces put in play on m by separate displacements u, v, w respectively $\parallel$ axes of x, y, z will be $A u$, $B v$, $C w$ respectively, and will act in the direction of the displacements; and $\therefore$ the whole initial force put in play on m by a displacement ρ, making $\angle$s α, β, γ with the axes, will be the resultant of the three, $\rho A \cos \alpha$, $\rho B \cos \beta$, $\rho C \cos \gamma$, and will $\therefore$

$$= \rho K = \rho \sqrt{A^2 \cos^2 \alpha + B^2 \cos^2 \beta + C^2 \cos^2 \gamma},$$

in a direction making $\angle$s λ, μ, ν with the axes, such that

$$K \cos \lambda = A \cos \alpha, \; K \cos \mu = B \cos \beta, \; K \cos \nu = C \cos \gamma,$$

and therefore not generally coincident with the direction of displacement.

It follows also from a comparison of some of the chief properties of the principal axes of rotation, that

1. The resolved part in the direction of an axis, inclined at

*s α, β, γ to the axes of elasticity, of the force put in play on m by a displacement $\rho \parallel$ it

$$= \rho \{A \cos^2 \alpha + B \cos^2 \beta + C \cos^2 \gamma\}:$$

2. The sum of such resolved forces in the directions of any three rectangular axes is constant

$$= \rho \{A + B + C\}:$$

3. The force produced by a displacement in the direction of one of the axes of elasticity is the greatest, and in that of another the least that can be produced in any one direction by the same displacement in that direction.

I am, Gentlemen, yours, &c.

St. John's College, Cambridge, Aug. 24, 1836. C. J.

VII. *On a new Theorem in Analysis.* *By the Rev.* Robert Murphy, *M.A.*

LAPLACE first gave a very simple and elegant demonstration* of the theorem generally known as Lagrange's, by taking the partial differential coefficients of u relative to x and a, from the equation $y = a + x \phi(y)$, where $u = f(y)$; and the simplicity of the process depends on the manner in which x enters this equation, namely, as a multiplier of a function of y.

I have considered a more general equation where x enters the function in any manner, viz. $y = a + \phi(x, y)$ and $u = f(y)$, and have obtained the following theorem:

When x is changed into $x + h$, in this equation y and consequently u are also changed; let the latter become U, and let $\phi(x + h, y) - \phi(x, y) = \Delta \phi$ for abridgement; the relation between U and u will then be

$$U = u + \Delta \phi \cdot \frac{du}{da} + \frac{d}{da}\left\{\frac{(\Delta \phi)^2}{1.2} \cdot \frac{du}{da}\right\}$$

$$+ \frac{d^2}{da^2}\left\{\frac{(\Delta \phi)^3}{1.2.3} \cdot \frac{du}{da}\right\} + \&c.$$

In proving this we shall use ϕ as a contraction of $\phi(x, y)$, and any *accented* symbol will be used to express the *partial* differential coefficient relative to x; thus instead of $\frac{d \cdot \phi(x, y)}{dx}$ we shall simply write ϕ'.

Now differentiating the proposed equation $y = a + \phi$ relative to x and a, we get

$$\frac{dy}{dx} = \phi' + \frac{d\phi}{dy} \cdot \frac{dy}{dx}$$

$$\frac{dy}{da} = 1 + \frac{d\phi}{dy} \cdot \frac{dy}{da},$$

from which we easily obtain

$$\frac{dy}{dx} = \phi' \cdot \frac{dy}{da};$$

and since $u = f(y)$, therefore

$$\frac{du}{dx} = \frac{du}{dy} \cdot \frac{dy}{dx}, \quad \frac{du}{da} = \frac{du}{dy} \cdot \frac{dy}{da},$$

whence

$$\frac{du}{dx} = \phi' \cdot \frac{du}{da} \quad . \quad . \quad . \quad (1.)$$

Thus far the process is exactly similar to Laplace's, but since ϕ' in the present case is not a function of y only, the remainder of the investigation becomes essentially different from his.

I put

$$\frac{d^n u}{dx^n} = P_{1,n} \frac{du}{da} + \frac{d}{da}\left(P_{2,n} \frac{du}{da}\right) + \frac{d^2}{da^2}\left(P_{3,n} \frac{du}{da}\right) + \&c., (2.)$$

and now proceed to find the value of the general symbol $P_{m,n}$, which is a function both of x and y.

Before differentiating this equation relative to x let it be observed that

$$\frac{d \cdot P_{m,n}}{dx} = P'_{m,n} + \frac{d \cdot P_{m,n}}{dy} \cdot \frac{dy}{dx}$$

$$= P'_{m,n} + \frac{d \cdot P_{m,n}}{dy} \cdot \phi' \frac{dy}{da}$$

$$= P'_{m,n} + \phi' \cdot \frac{d \cdot P_{m,n}}{da}$$

and also that

$$\frac{d}{dx} \cdot \frac{du}{da} = \frac{d}{da} \cdot \frac{du}{dx} = \frac{d}{da}\left(\phi' \frac{du}{da}\right)$$

by equation (1.); from both of which it follows that

$$\frac{d}{dx}\left(P_{m,n} \frac{du}{da}\right) = \left(P'_{m,n} + \phi' \cdot \frac{d P_{m,n}}{da}\right) \cdot \frac{du}{da}$$

$$+ P_{m,n} \frac{d}{da}\left(\phi' \frac{du}{da}\right)$$

$$= P'_{m,n} \cdot \frac{du}{da} + \frac{d}{da}\left(P_{m,n} \cdot \phi' \frac{du}{da}\right).$$

Applying this formula to differentiate equation (2.) relative to x, we get

$$\left.\begin{aligned}\frac{d^{n+1} u}{d x^{n+1}} = P'_{1,n} \frac{du}{da} + \frac{d}{da}\left(P'_{2,n} \frac{du}{da}\right) \\ + \frac{d^2}{da^2}\left(P'_{3,n} \frac{du}{da}\right) + \&c. \\ + \frac{d}{da}\left(P_{1,n} \phi' \frac{du}{da}\right) \\ + \frac{d^2}{da^2}\left(P_{2,n} \phi' \frac{du}{da}\right) + \&c.\end{aligned}\right\} \quad . \quad . \quad . \quad (3.)$$

But if we write $n + 1$ for n in the formula (2.), we also have

$$\frac{d^{n+1} u}{d x^{n+1}} = P_{1,n+1} \cdot \frac{du}{da} + \frac{d}{da}\left(P_{2,n+1} \frac{du}{da}\right) + \frac{d^2}{da^2}\left(P_{3,n+1} \frac{du}{da}\right) + \&c.,$$

which compared with the preceding expression shows the following law for the formation of the functions $P_{m,n}$, viz.

$$P_{m,n+1} = P'_{m,n} + \phi' \cdot P_{m-1,n} \quad . \quad . \quad . \quad . \quad (4.)$$

And since $P_{m,1}$ is known, (for $P_{1,1} = \phi'$, $P_{2,1} = 0$, $P_{3,1} = 0$, &c.,) we can thus form successively the quantities $P_{m,2}$, $P_{m,3}$, &c., and the general law of these quantities may be thus found.

Put

$$1.2.3 \ldots m\, P_{m,n} = (\phi^m)^{\prime\prime\prime(n)} - A_m \cdot \phi\, (\phi^{m-1})^{\prime\prime\prime(n)} + B_m \phi^2\, (\phi^{m-2})^{\prime\prime\prime(n)} - C_m \phi^3\, (\phi^{m-3})^{\prime\prime\prime(n)} + \&c.$$

Hence

$$\left.\begin{aligned}1.2.3 \ldots m\, P'_{m,n} = (\phi^m)^{\prime\prime(n+1)} - A_m\, \phi\, (\phi^{m-1})^{\prime\prime(n+1)} \\ + B_m\, \phi^2\, (\phi^{m-2})^{\prime\prime(n+1)} - C_m\, \phi^3\, (\phi^{m-3})^{\prime\prime(n+1)} + \&c. \\ - A_m\, \phi'\, (\phi^{m-1})^{\prime\prime\prime(n)} + 2\, B_m\, \phi\, \phi'\, (\phi^{m-2})^{\prime\prime\prime(n)} \\ - 3\, C_m\, \phi^2\, \phi'\, (\phi^{m-3})^{\prime\prime\prime(n)} + \&c.\end{aligned}\right\}$$

Also

$$1.2.3 \ldots m\, \phi'\, P_{m-1,n} = m\, \phi'\, (\phi^{m-1})^{\prime\prime\prime(n)} - m\, A_{m-1}\, \phi\, \phi'\, (\phi^{m-2})^{\prime\prime\prime(n)} + m\, B_{m-1}\, \phi^2\, \phi'\, (\phi^{m-3})^{\prime\prime\prime(n)} + \&c.$$

And by equation (4.) the sum of these expressions must be equal to $1.2.3 \ldots m\, P_{m,n+1}$, that is,

$$= (\phi^m)''^{(n+1)} - A_m \phi\, (\phi^{m-1})''^{(n+1)} + B_m \phi^2 (\phi^{m-2})''^{(n+1)}$$

$$- C_m \phi^3 (\phi^{m-3})''^{(n+1)} \text{ \&c.}$$

From whence we have

$$A_m = m,\ 2\, B_m = m\, A_{m-1} \text{ or } B_m = \frac{m\,(m-1)}{2}$$

$$3\, C_m = m\, B_{m-1} \text{ or } C_m = \frac{m\,(m-1)\,(m-2)}{2.3} \text{ \&c.};$$

and therefore

$$1.2.3 \ldots m\, P_{m,n} = (\phi^m)'''^{(n)} - m\, \phi\, (\phi^{m-1})'''^{(n)}$$

$$+ \frac{m\,(m-1)}{2} \varphi^2 (\varphi^{m-2})'''^{(n)} + \text{\&c.}, \quad . \quad . \quad . \quad (5.)$$

which series terminates at the m^{th} term, since $(\varphi^{m-m})''^{(n)} = 0$.

If we put 1, 2, 3, &c. successively for m in this formula, and substitute in the expression (2), stopping at $P_{n,n}$, we should have $\frac{d^n u}{d x^n}$ explicitly obtained; but there is no necessity for this.

Again, since $\Delta \phi = \varphi\,(x + h, y) - \phi\,(x, y)$, therefore

$$(\Delta\, \varphi)^m = \{\varphi\,(x + h, y)\}^m - m\, \varphi\,(x, y)\, \{\varphi\,(x + h, y)\}^{m-1}$$

$$+ \frac{m\,(m-1)}{2} \{\varphi\,(x, y)\}^2 \{\varphi\,(x + h, y)\}^{m-2} - \text{\&c.}$$

Each term in this series may be expanded according to powers of h by Taylor's theorem; and if we take the coefficient of $\frac{h^n}{1.2.3 \ldots n}$ in each term, the coefficient of $\frac{h^n}{1.2.3 \ldots n}$ in $(\Delta\, \varphi)^m$ is exactly the same as the series (5.).

By this comparison we find that $P_{m,n}$ is the coefficient of $\frac{h^n}{1.2.3 \ldots n}$ in the expansion of $\frac{(\Delta\, \phi)^m}{1.2.3 \ldots m}$.

Recur now to the series (2.), and we get

$\frac{d^n u}{d x^n}$ = the coefficient of $\frac{h^n}{1.2.3 \ldots n}$ in the series following, viz.

$$\Delta\, \varphi \,.\, \frac{d u}{d a} + \frac{d}{d a} \left\{ \frac{(\Delta\, \varphi)^2}{1.2} \cdot \frac{d u}{d a} \right\} + \frac{d^2}{d a^2} \left\{ \frac{(\Delta\, \varphi)^3}{1.2.3} \cdot \frac{d u}{d a} \right\} + \text{\&c.}$$

But by Taylor's theorem the same quantity is the coefficient of $\frac{h^n}{1.2.3 \ldots n}$; in the expansion for $U - u$, we therefore find the theorem announced at the commencement, viz.

$$U = u + \Delta \phi \cdot \frac{d u}{d a} + \frac{d}{d a}\left\{\frac{(\Delta \phi)^2}{1.2} \cdot \frac{d u}{d a}\right\}$$
$$+ \frac{d^2}{d a^2}\left\{\frac{(\Delta \phi)^3}{1.2.3} \cdot \frac{d u}{d a}\right\} + \&c.$$

We have not space here to point out the applications of this general theorem, and shall therefore close this paper with two remarks.

First, if $\phi(x, y)$ be of the form $x \phi(y)$, then $\Delta \phi = h \phi y$; we have then

$$U = u + h \phi(y) \cdot \frac{d u}{d a} + \frac{h^2}{1.2} \frac{d}{d a}\left\{(\phi y)^2 \cdot \frac{d u}{d a}\right\}$$
$$+ \frac{h^3}{1.2.3} \frac{d^2}{d a^2}\left\{(\phi y)^3 \cdot \frac{d u}{d a}\right\} \&c.;$$

and if we suppose $x = 0$, then $y = a$, and U is then the value of $f(y)$ determined from the equation $y = a + h \phi(y)$: we thus fall on Lagrange's theorem.

Secondly, that if the proposed equation were

$$y = F\{a + \phi(x, y)\} \text{ and } u = f(y),$$

the fundamental equation (1.) would remain the same, and therefore this theorem admits of the same extension that Laplace gave to Lagrange's. R. M.

VIII. *On the Property of the Parabola demonstrated by* Mr. Lubbock *in the Phil. Mag. for August. By* A Correspondent.

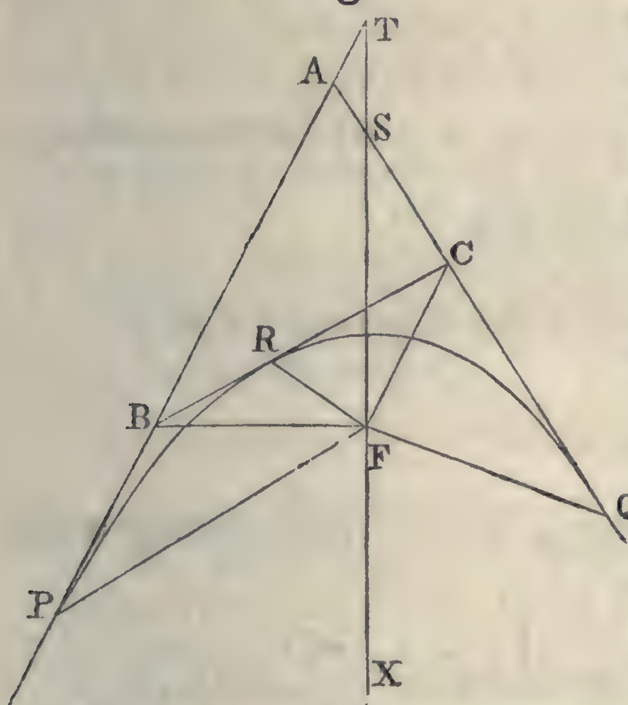
To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the last [August] Number of your Journal there is a demonstration by Mr. Lubbock of a very beautiful property of the parabola. Mr. L. does not seem to be aware that this problem, which he ascribes to the French, is in fact due to Prof. Wallace of Edinburgh, who before the end of the last century communicated it to Mr. Leybourne, by whom it was published as a prize question. Not having the book at present, I cannot tell in which volume of the Repository it is to

be found, but, if I recollect rightly, there is more than one solution of it. The most simple way of considering the question is undoubtedly by geometry, as the following demonstration will testify.

Fig. 1.



Let A P, A Q, B C be the three tangents intersecting in A B C. Draw the diameter F S T, meeting A Q and A P in S and T. Join F P, F Q. Then, by a known property of the parabola, P F X = 2 P T F, Q F X = 2 Q S F. Also B A C = P T F + Q S F. Therefore P F Q = 2 B A C. Again, join F B, F C, F R. By a property of the parabola P F B = B F R and Q F C = C F R. Therefore the salient angle P F Q = 2 B F C, and all the angles round F or 4 right angles = 2 (B A C + B F C), or B A C + B F C = 2 right angles, and therefore a circle can be described round A B C F.

But if an analytical proof of the theorem be required, the following, by Mr. Greatheed, of Trinity College, deserves attention from its elegance and brevity.

Let the curve be referred to one of the tangents and the diameter through the point of contact: then its equation is $y^2 = 4\,m_1\,x$; and the equations to the two other tangents, whose points of contact are $x_1\,y_1\;x_2\,y_2$, are

$$y\,y_1 = 2\,m_1\,(x + x_1), \quad y\,y_2 = 2\,m_1\,(x + x_2).$$

These tangents will cut the axis of y at distances $\frac{y_1}{2}$, $\frac{y_2}{2}$; and the coordinates of their point of intersection are

$$\frac{y_1\,y_2}{4\,m_1}, \quad \frac{y_1 + y_2}{2}.$$

If θ be the angle between the axes, the equation to a circle is

$$x^2 + 2\,x\,y\cos\theta + y^2 + a\,x + b\,y + c = 0.$$

When $x = 0$, $y^2 + b\,y + c = 0$.

But the roots of this must be $\frac{y_1}{2}$, $\frac{y}{2}$. Therefore

$$b = -\frac{y_1 + y_2}{2}, \quad c = \frac{y_1\,y_2}{4}.$$

In order that the circle may pass through the third poin we must have

$$\frac{y_1^2\, y_2^2}{16\, m_1^2} + \frac{y_1 y_2 (y_1 + y_2)}{4\, m_1} \cos\theta + \frac{(y_1 + y_2)^2}{4} + a\,\frac{y_1\, y_2}{4\, m_1}$$

$$- \frac{(y + y_2)^2}{4} + \frac{y_1\, y_2}{4} = 0,$$

or $\dfrac{y_1\, y_2}{4\, m_1} + (y_1 + y_2)\cos\theta + a + m_1 = 0$, which determines a.

Hence the equation to the circle is

$$x^2 + 2\,x\,y\cos\theta + y^2 - \left(\frac{y_1 y_2}{4\, m_1} + (y_1 + y_2)\cos\theta + m_1\right)$$

$$x - \frac{y_1 + y_2}{2}\,y + \frac{y_1 y_2}{4} = 0.$$

When it cuts the parameter $x = m_1$. Substituting which we get

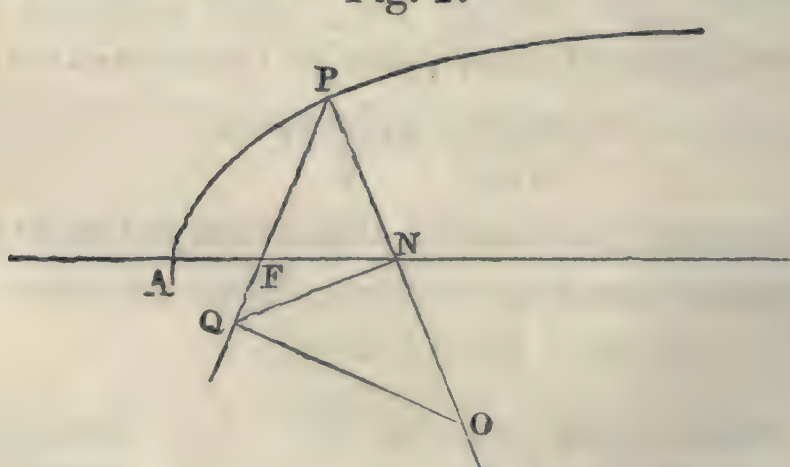
$$y^2 + \left(2\,m'\cos\theta - \frac{y_1 + y_2}{2}\right)y - m_1\,(y_1 + y_2)\cos\theta = 0\,;$$

the roots of which are obviously $-\,2\,m_1\cos\theta$, and $\dfrac{y_1 + y_2}{2}$. The first of these shows that it passes through the focus.

We thus see that by the use of oblique coordinates the problem is simplified in the most remarkable manner, and there is no necessity for employing the troublesome method of finding the focus, which Mr. Lubbock's solution requires.

As in some degree connected with this subject, I would call your attention to two properties of the conic sections, which seem scarcely, if at all, known, and are yet deserving of attention. The first is due to Keil, and furnishes an easy method of finding the centre of curvature of a conic section.

Fig. 2.



Let A F N be the axis; F the focus; P N a normal meeting the axis in N; N Q a perpendicular to the normal meet-

ing the chord through the focus in Q; Q O perpendicular to P Q and meeting the normal in O: then O is the centre of curvature.

The other theorem I found in some manuscripts of the same period, but it was probably never published. It is as follows: If a sphere be described round the vertex of a cone as centre, the latera recta of all sections of the cone made by planes touching the sphere will be constant, and equal to the radius of the sphere multiplied by the tangent of half the vertical angle of the cone. I will not add the proofs of these theorems, as they are attended with no difficulty.

The insertion of this notice will much oblige yours, &c.,

Trinity College, Cambridge. G.

IX. *Concise Demonstration of the Property of the Parabola in the Phil. Mag. for August. By the Rev.* Hamnett Holditch, *Fellow of Caius College, Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the last [August] Number of your Magazine is an elaborate and ingenious demonstration of the theorem thus stated by M. Poncelet, "Un triangle étant circonscrit à une parabole, si on lui circonscrit à son tour une circonférence de cercle, elle passera nécessairement par le foyer même de la courbe." I venture to propose for insertion the following very short demonstration of this theorem: Let $(x_1\, y_1)$, $(x_2\, y_2)$, $(x_3\, y_3)$ be the points of the parabola to which the tangents are drawn; $(X_{12}\; Y_{12})$ the point where the tangents to the points $(x_1\, y_1)$ $(x_2\, y_2)$ meet; then if $y^2 = l\, x$ be the equation to the parabola, the equation to the tangent at the first point is

$$2\, y\, y_1 = l\, x + l\, x_1 \quad \therefore 2\, Y_{12}\, y_1 = l\, X_{12} + l\, x_1$$

$$\text{and} \quad 2\, Y_{12}\, y_2 = l\, X_{12} + l\, x_2;$$

and we have therefore

$$X_{12} = \frac{y_1\, y_2}{l} \quad \text{and} \quad Y_{12} = \frac{y_1 + y_2}{2}.$$

Let $x^2 + y^2 + a\, x + b\, y + c = 0$ be the equation to the circle passing through the three points,

$$\therefore X^2_{12} + Y^2_{12} + a\, X_{12} + b\, Y_{12} + c = 0$$

$$\text{and} \quad X^2_{23} + Y^2_{23} + a\, X_{23} + b\, Y_{23} + c = 0;$$

and subtracting the latter from the former, and dividing by $Y_{12}-Y_{23}$, we have

$$\overline{X_{12}+X_{23}} \cdot \frac{X_{12}-X_{23}}{Y_{12}-Y_{23}} + Y_{12} + Y_{23} + a \cdot \frac{X_{12}-X_{23}}{Y_{12}-Y_{23}} + b = 0;$$

but $Y_{12} - Y_{23} = \dfrac{y_1 - y_3}{2}$ and $X_{12} - X_{23} = \dfrac{y_1 y_2 - y_2 y_3}{l}$,

$$\therefore \frac{X_{12}-X_{23}}{Y_{12}-Y_{23}} = \frac{2y_2}{l}, \quad X_{12}+X_{23} = \frac{y^2}{l} \cdot \overline{y_1 + y_3},$$

$$\text{and } Y_{12} + Y_{23} = \frac{y_1 + 2y_2 + y_3}{2}:$$

and therefore

$$\frac{\overline{y_1 + y_3} \cdot 2y^2_2}{l^2} + \frac{y_1 + 2y_2 + y_3}{2} + \frac{2ay_2}{l} + b = 0.$$

And similarly

$$\frac{\overline{y_2 + y_1} \cdot 2y^2_3}{l^2} + \frac{y_2 + 2y_3 + y_1}{2} + \frac{2ay_3}{l} + b = 0,$$

$$\therefore a = -\left(\frac{y_1 y_2 + y_1 y_3 + y_2 y_3}{l} + \frac{l}{4}\right)$$

$$\text{and} \quad b = \frac{2y_1 y_2 y_3}{l^2} - \frac{y_1 + y_2 + y_3}{2};$$

and substituting these values in the equation $X^2_{12} + Y^2_{12} + aX_{12} + bY_{12} + c = 0$, we have

$$c = \frac{y_1 y_2 + y_1 y_3 + y_2 y_3}{4}.$$

To find where the circle cuts the axis make $y = 0$;

$$\therefore x^2 + ax + c = 0, \text{ or}$$

$$x^2 - \left(\frac{y_1 y_2 + y_1 y_3 + y_2 y_3}{l} + \frac{l}{4}\right) \cdot x + \frac{y_1 y_2 + y_1 y_3 + y_2 y_3}{4} = 0,$$

or

$$x^2 - \left(X_{12} + X_{13} + X_{23} + \frac{l}{4}\right) \cdot x + \frac{X_{12} + X_{13} + X_{23}}{4} \cdot l = 0,$$

or $\left(x - \overline{X_{12} + X_{23} + X_{13}}\right) \cdot \left(x - \dfrac{l}{4}\right) = 0;$

$$\therefore 0 = x - X_{12} + X_{23} + X_{13} \quad \text{and} \quad 0 = x - \frac{l}{4},$$

and the latter factor shows that the circle passes through the focus of the parabola.

August, 13, 1836.

X. *On the Classification of Vegetables. By the Rev.* Patrick Keith, *F.L.S.**

CLASSIFICATION may be defined to be the arranging of the productions of nature in a system, so as either to show the mutual relation which the several subjects or groups of subjects bear to one another in the scale of being, or merely to facilitate our ascertaining of the names which have been imposed upon them by their discoverers or others. In the former case the classification is natural, and is exemplified, as far as regards plants, in the arrangements of Jussieu. In the latter case the classification is artificial, and is exemplified, as far as regards plants, in the arrangements of Linnæus. After all, this distinction, as M. Raspail well observes †, is more trenchant in expression than in reality, as every good classification will have something in it both of the one quality and of the other.

Linnæus, as every one knows, founded his classes chiefly upon the number of the stamens, and his orders upon the number of the styles. But as distinctions arising from number merely are of themselves entirely artificial, so is the system that is founded upon them. They do not necessarily give any indication of natural groups, and yet it is singular enough that this artificial system has brought together several tribes of plants that are perfectly natural, as the Grasses, the Papilionaceæ, the Cruciferæ. Still the study of it gives the disciple but little knowledge of a plant beyond the name. He counts his stamens and pistils, and becomes a perfect master of classes and orders; but of the interior and more recondite parts and properties, whether of stem or of flower, by which different genera are allied and connected together, that is, of the natural affinities of plants, he knows nothing. Yet this, as Linnæus himself admitted, is the grand end and aim of all botanical investigation. "*Methodus naturalis, hinc, ultimus finis Botanices est et erit* ‡." If he had put *fuit* into his maxim it would still have been equally true, for all inventors of systems, even from the earliest times, have had an eye to a natural method. The very division of plants into herbs, shrubs, and trees, the oldest and most popular of all, as well as the most humble in its pretensions, is founded upon a presumed or apparent affinity between the subjects of its different groups. This division is at least as ancient as the age of Theophrastus, if not, rather, as ancient as that of Moses, who speaks of grass, herb, and tree as comprehending and exhausting the whole of

* Communicated by the Author.

† *Chim. Organ.*, p. 84.

‡ *Phil. Bot.*, p. 137.

the vegetable kingdom. In later times,—that is, after the period of the decline of learning, and interval of the dark ages,—when science began to revive and the seeds of sound investigation to take root, botanists began also to introduce into their lovely study the principles of sound arrangement. Cæsalpinus, Ray, and Tournefort may be named as individuals who contributed much towards the introduction of a natural system, although they were not fortunate enough to stumble upon the true foundation on which alone it can be made to rest. Linnæus himself lent his able aid, and in his Fragments of a Natural Method exhibited to the botanical world the proofs of his qualification for the task. There is no saying how far he might have proceeded in the prosecution of his plan, if it had not been that he was so much occupied in the perfecting of his artificial method,—a mere stepping-stone to his natural method,—that he could not find time for the perfecting of both. But by thus showing his disciples an easy and royal road to learning, he unfortunately, and without thinking of it, adopted the very means of preventing the student from entering upon, or following up, the intricate and uninviting path that leads by slow degrees to the elevated station from which he may discern the beauties, and appreciate the value, of a natural arrangement.

Yet this laborious and uninviting task was at last undertaken, and prosecuted with a success beyond all that could have been expected. The principal part of the achievement is usually ascribed to M. Bernard de Jussieu, and a subordinate part to his nephew, M. A. Laurent de Jussieu, who is represented as being merely the editor of the writings of his uncle. This erroneous notion seems to have been taken up hastily by the contemporary botanists of this country, and handed down from one to another without much inquiry, till at last it attracted the notice of M. Adrien de Jussieu, as occuring in the Introduction to the *Flora Indica* of Messrs. Wight and Arnott, and called forth a statement that settles the respective claims of the uncle and nephew, and corrects the error that had become too prevalent with regard to them. (*Annales des Sci. Nat.*, Nov. 1834.)

From this we learn that the uncle suggested indeed some of the grand outlines of the *Genera Plantarum*, but gave no filling up. He adopted the germination of the seed and the relative disposition of the sexual organs as the only true ground of all systematic arrangement. He formed families, but not classes, and left, in short, nothing in writing but the manuscript catalogues of the garden of Trianon. He died in 1777.

But the *Genera Plantarum secundum Ordines Naturales dis-*

posita, as published in 1789, is something more than a mere catalogue, and M. A. Laurent de Jussieu is something more than its mere editor: he is, in fact, its author. He could not have been the editor of the writings of one who wrote nothing. He could not have gathered all that was necessary to the composition of the *Genera Plantarum* from a few occasional conversations with his uncle. He could not have produced that *chef-d'œuvre* of botanical and logical arrangement without years of close and previous study. Besides, many facts necessary to its final completion were not even known at the time of his uncle's death. Hence we see why the work was not ready for publication before 1789. Hence we see what portion of it is to be ascribed to the uncle and what to the nephew; and we must beware of detracting from the merits of the one for the purpose of enhancing the merits of the other.

In this profound and elaborate work the subjects of the vegetable kingdom are distributed into three grand groups, Acotyledonous, Monocotyledonous, and Dicotyledonous plants. The sections are founded on the peculiarities of the corolla, and the classes on the insertion of the stamens. Still the advocates of the sexual system say that the method of Linnæus is not more artificial than that of Jussieu, whom they accuse of founding his sections merely on number, as Linnæus founds his classes and orders *. But it should be recollected that it is under very different circumstances. Linnæus selects a single species of organ,—the stamens—and all plants furnished with the same number of stamens are thrust into the same class without reserve, let their natural affinities be what they will. Indeed, natural affinities are not so much as looked for. Thus you have the Asperifoliæ and the Umbelliferæ, the Bugloss and the Bulbocastanum, associated in the same class, without any connecting link, apparent or presumptive, beyond that of their having the same number of stamens; and thus you are under the necessity of separating the single genus Anthoxanthum from the natural family of the Grasses, because it happens to have but two stamens to its flower instead of three, which the rest of the grasses have. In the class Dodecandria the stamens should be twelve, but by special allowance they may be from eleven to nineteen. The orders exhibit the same incongruities. In Diandria Monogynia, you have Enchanter's Nightshade and Common Ash placed side by side; and in Pentandria Digynia, you have Cuscuta and Ulmus. The styles of Icosandria Pentagynia are by special privilege also allowed to be from two to five. Yet these incongruities are not to be se-

* Roscoe on Arrangements, Linn. Trans., vol. xi. [or Phil. Mag. and Annals, N.S., vol. vii.—Edit.]

verely censured, seeing that the sexual system is actually and professedly artificial.

But in the system of Jussieu several important traits of affinity are already determined before the class is fixed. All plants composing the classes of the first grand group are already connected by the link of their being cotyledonous, that is, by a character founded on the structure of the embryo, and its mode of growth. All plants composing the classes of the Dicotyledons and Monocotyledons are further connected by their being respectively exogenous or endogenous, accordingly as they belong to the former or to the latter division, that is, by a character founded on the structure of the stem and its mode of growth. These characters are evidently and essentially natural. The Dicotyledons are subdivided into minor groups, upon the ground of their being furnished with a calyx and corolla, or with a calyx only, a character found to be of the greatest importance in bringing together natural orders, though not infallible; and the subdivisions are distributed into sections, upon the ground of their being polypetalous, monopetalous, apetalous, or anomalous. If these last characters are not absolutely natural, they are at least absolutely necessary to give facility to the investigations of the student, and are to be admitted till better characters are discovered; and if you say that they are founded on number merely, it is not exactly so. It is upon structure rather than upon number, a character of more value. For the distinction lies between a corolla, the petals of which are free, and a corolla, the petals of which are united, or it depends upon the absence of a corolla altogether.

Lastly, from the several sections, the classes themselves, which are fifteen in number, derive their immediate origin, upon the principle of the insertion of the stamens, as being hypogynous, perigynous, or epigynous, characters evidently affinial*, and very available to Linnæus in the circumscription of his 12th, 13th, and 20th classes; for how, without their aid, could he have brought together so many plants connected by natural affinities where his styles and stamens might not have been easily counted? Number is no doubt the work of nature, as well as other characters, but it is found to be liable to great mutability, by an abortion, or by an undue multiplication of parts, and is consequently not to be depended upon in the circumscribing of orders or of genera. It is but a fallacious mark at the best, if taken by itself; for although the genera belonging to a natural order may have all the same number of styles and stamens, yet all plants having the same number of styles and stamens do not belong to the same natural order. Hence

* [See Lond. and Edinb. Phil. Mag., vol. v. p. 206, note.—Edit.]

it is not from any single trait of resemblance that natural orders are to be determined, but from the sum of the affinities discoverable in the number, form, structure, and position of the several organs composing the stem, leaf, flower, fruit, or seed, the organs last developed being regarded as the most important.

Such is the sure foundation on which the system of Jussieu is built; but still its merits were not at first duly appreciated, whether in France or in other countries. The *éclat* which the name of Linnæus gave to the sexual system was such that no system standing in opposition to it was likely to succeed. Its novelty, its facility, its beauty, were attractions that could not be resisted. Hence Jussieu had many prejudices to encounter, and a host of adversaries to discomfit, before he could divest the natural system of the dreaded difficulties which the study of it seemed to involve, and to present it to the botanical student in a fair and favourable light. Yet, in spite of all obstacles, its superiority to every other system forced itself at last upon the notice of botanists, and began to make converts even from among the disciples of Linnæus. In France, the late M. Richard, the Chevalier Aubert du Petit Thouars, M. Mirbel, and the *élite* of the French school were among the first to enrol themselves under the standard of Jussieu; in Germany, Kunth, Von Martius; and in Switzerland, M. DeCandolle. But the botanist whom we regard as having distinguished himself the most conspicuously in the elucidating and perfecting the system of Jussieu, is our celebrated countryman and fellow-Linnæan Dr. Robert Brown, as may be seen by consulting his *Prodromus Floræ Novæ Hollandiæ*, or his papers published in the Linnæan Transactions, particularly that on the Proteaceæ of Jussieu, and on the organs and mode of fecundation in the Orchideæ and Asclepiadeæ; together with that on the genus Rafflesia, followed by a paper read at a meeting of the Society, June 17, 1834, in which he completes his account of *Rafflesia Arnoldi*, and creates a new order, which he denominates Rafflesiaceæ*; all discovering a profundity of research, an acuteness of discrimination, and a peculiarity of tact in seizing the essential character that connects or disunites the subjects of his investigation, which, without his assuming anything of bold or of arrogant pretension, have elevated him to a rank beyond that of all his competitors, and established his claim to the compliment that was formerly paid to Linnæus, namely, that of his being emphatically, and in the estimation of botanists themselves, *Botanicorum facile princeps*†. We

* [An abstract of Dr. Brown's paper was given in Lond. and Edinb. Phil. Mag., vol. v. p. 70.—Edit.]

† Arnott, *Encyc. Brit.*, art. Bot.

cannot on this occasion pass by without notice Mr. Professor Don, of King's College, London, whom we regard as occupying a very elevated station among the botanists of this country, and second only to Dr. Brown in the number of valuable contributions with which he has enriched the Transactions of the Linnæan Society, all tending to advance and to illustrate the system of Jussieu.

[To be continued.]

XI. *On the Laws of Crystalline Reflexion.* By James MacCullagh, *Fellow of Trinity College, Dublin.**

In a Number of Poggendorff's *Annalen* (No. 6, for 1836,) which reached Dublin late in November, there are some remarks by M. Seebeck on a paper of mine which appeared in the last February Number of this Journal, (vol. viii. p. 103). That paper contains a general theory of reflexion at the surfaces of crystallized media; and M. Seebeck, in comparing the results with his own experiments, has fully confirmed some of my formulæ, while he has shown that others are defective. I have therefore been obliged to revise my theory, and I have ascertained that it was vitiated by the introduction of a certain relation among the quantities denominated pressures, which, following the example of M. Cauchy, I had supposed to be concerned in the problem. This relation I had observed to hold in the case of singly refracting media, and I concluded, without any other reason, that it would hold good generally. But though it led to the correct formula for the polarizing angles in different azimuths, it was nevertheless arbitrary and unfounded; and therefore it is now banished entirely from the investigation, the place which it occupied being supplied by the natural and simple law of the preservation of *vis viva*, while everything else remains as before. I hope the imperfection of my first essay will be excused, when it is considered that the erroneous proposition bears but a small proportion to the whole theory, and moreover, that the general problem, which I undertook to resolve, is one that has not been attempted by any other person, although the want of a solution has long been felt. The difficulties which we have to deal with, in entering upon this problem, are not mere mathematical difficulties, but difficulties arising from the want of first principles; and, in physical questions of this kind, where we must, at the outset, have recourse to conjecture, in order to supply the very principles of our reasoning, it can hardly be expected that the whole truth should be divined at once. I think, however,

* Communicated by the Author.

that I have now obtained a true mechanical theory; and if so, it will help to decide, not only the question immediately before us, but also the other much-disputed, though more elementary, questions concerning the density of the æther in different media, and the direction of the vibrations in polarized light. In fact, a particular supposition respecting each of the latter questions is included in my theory, the several principles of which, making the single alteration that has been mentioned, I shall here enumerate:

1. The density of the æther is the same in all media.

2. The vibrations of plane-polarized light are parallel to the plane of polarization.

3. The *vis viva* is preserved.

4. The vibrations are equivalent at the common surface of two media.

To these may be added the definition of the polarizing angle of a crystal; namely, the angle of incidence at which the plane of polarization of the reflected ray becomes independent of the plane of polarization of the incident ray. At the polarizing angle, the former plane does not, in general, coincide with the plane of reflexion, but makes with it a small angle which may be called the *deviation.*

It is curious that, about a year and a half ago, I employed these four principles, precisely as I have now enumerated them, in deducing Fresnel's well-known laws of reflexion for ordinary media; but I did not then apply the law of *vis viva* to crystals, because my mind was preoccupied by the notion that there existed some relation among the pressures. This notion I had taken up from reading a little paper, by M. Cauchy, in the *Bulletin des Sciences Mathématiques* for July 1830; and by combining such a relation with the three conditions afforded by my own law of *equivalent vibrations,* I had actually obtained, for the polarizing angles in different azimuths, a formula (that marked (5.) in my former paper,) which I found to agree very well with Sir David Brewster's experiments, and which M. Seebeck has found to agree still better with his own.

The formula for the polarizing angle is obtained by equating two values of the deviation; and it is remarkable that the very same formula comes out in my present theory, although the values of the deviation are entirely different. Referring, for brevity, to the notation* of my former paper, I find, for the case of a uniaxal crystal,

$$\tan\beta = \cos(i+\phi)\tan\theta\,, \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (a.)$$

* Erratum in former paper, vol. viii. p. 106, line 2 from bottom, for $\frac{\pi}{2}+\delta$ *read* $\frac{\pi}{2}-\delta$.

$$-\tan \beta' = \cos(i+\phi') \cot \theta' + (a^2-b^2) \frac{\sin \psi' \cos \psi' \sin^2 i}{\sin \theta' \sin(i-\phi')} \qquad (b.)$$

These equations (*a.*) and (*b.*) are to be substituted for equations (2.) and (3.), which are the equations that M. Seebeck found to be at variance with his experiments.

By means of formula (5.), equation (*a.*) becomes

$$\beta = \frac{K}{2} \sin 2q \sin(p-\phi), \quad \ldots\ldots\ldots\ldots \quad (c.)$$

from which the deviation in any azimuth may be readily calculated. The azimuth (as M. Seebeck reckons it) begins when $\theta = 0$, and p is then positive. This formula (*c.*) perfectly represents the experiments of M. Seebeck on Iceland spar. The corresponding expressions for biaxal crystals may be easily deduced, and will be given in a paper which I am preparing to lay before the Royal Irish Academy.

At the time of my last communication I was not aware that the case in which the plane of incidence is a principal section of the crystal (or the azimuth $= 0$,) had been solved by M. Seebeck, and that formula (7.), which I regarded as my own, had been obtained by him long before.

It remains to say a word respecting the new principle of equivalent vibrations, the most important, perhaps, of all, as it is certainly the simplest that can be imagined. If we conceive an æthereal molecule situated at the common surface of two media, it would seem that its motion ought to be the same, whether we regard the molecule as belonging to the first medium or to the second. Now the incident and reflected vibrations are superposed in the first medium, and the refracted vibrations in the second; and therefore we may infer (when the phase is not changed by reflexion or refraction), that *if the incident and reflected vibrations be compounded, like forces acting at a point, their resultant will be the same, both in length and direction, as the resultant of the refracted vibrations similarly compounded.* This is the law of equivalent vibrations, and it gives, at once, three equations. A fourth equation is afforded by Fresnel's law of the *vis viva*; and thus we have the four conditions necessary for a general solution of the problem. From the principle of equivalent vibrations, as we have stated it, it follows that the vibrations resolved parallel to the separating surface are equivalent in the two media; and in fact, this part of the general principle was assumed by Fresnel; but the other part, namely, that the vibrations perpendicular to the separating surface are equivalent, was not assumed by him, nor is it by any means true in his theory. It appears then that three conditions only are afforded by the

hypotheses which Fresnel successfully employed in solving the problem of reflexion from ordinary media. These hypotheses, therefore, are not sufficient when applied to crystals; except, indeed, in the case before alluded to, where the azimuth = 0, which has been solved by M. Seebeck. It should be observed, that though the reasons which I have assigned for the principle of equivalent vibrations are extremely simple, yet it was not by such simple reasoning that I was led to it originally.

Trinity College, Dublin, Dec. 13, 1836.

XII. *Researches in Organic Chemistry.—First Series. Contributions to the History of Pyroxylic Spirit and of its derived Combinations.* By Robert J. Kane, *M.D., M.R.I.A.**

MY residence in Giessen during the summer of the present year [1836] allowed me to continue under the guidance of Professor Liebig the examination of pyroxylic spirit, and the compounds obtained from it, which I had commenced prior to my becoming acquainted with the results of Dumas and Peligot, and of which a portion was presented to the British Association assembled in Dublin during August 1835†.

As sent into commerce pyroxylic spirit is contaminated by the presence of a volatile oily material, from which it is extremely difficult to obtain it free. Dumas states that the impurities are completely separated by distillation with fresh-burned lime, a process which in my hands at least has never perfectly succeeded. The pyroxylic spirit thus obtained is always rendered milky by admixture with water, and consequently unfit for accurate examination. Caustic potash and sulphuric acid were found equally unsuccessful in removing this impurity, from which, however, the pyroxylic spirit is obtained free by the following simple process.

Chloride of calcium being dissolved in pyroxylic spirit in considerable quantity, so much heat is evolved that the liquid if in considerable mass rises to its boiling point, and on cooling there is deposited a compound of pyroxylic spirit with chloride of calcium in large brilliant six-sided tables, like those of acetate of zinc. This compound requires for its decomposition a temperature much higher than that of boiling water. If therefore, the rough pyroxylic spirit be saturated with chloride of calcium and distilled, the excess of spirit and the oil come over, and are collected as long as by temperature of a water-bath the distillation continues. When this has

* Communicated by the Author.

† [An abstract of Dr. Kane's paper presented to the British Association, will be found in Lond. & Edinb. Phil. Mag., vol. vii. p. 397, and a notice of the results obtained by MM. Dumas and Peligot at p. 427 of the same volume, of which see also p. 395.—Edit.]

ceased, a quantity of water is to be introduced into the retort equal in volume to that of the pyroxylic spirit employed. The water replacing the spirit in the combination with chloride of calcium, the latter fluid is set free and distils, perfectly free from oil, but mixed with water, from which it can readily be separated by one or two distillations over recently ignited lime. The substance thus obtained was employed in the following experiments:

The boiling point at 0·744 met. barom. press. was 60° cent.

Analysed with the apparatus of Liebig 0·765 gramme of material gave

Water.........	= 0·853
Carbonic acid ...	1·042

From whence follows the composition:

Carbon... =	37·66	Dumas's theory gives 2 car. =	37·97
Hydrogen	12·39	8 hyd.	12·40
Oxygen...	49·95	2 oxy.	49·63
	100·		100·

By the following experiment the specific gravity of the vapour was determined by the method devised by Dumas:

Excess of weight of balloon filled with air over that of the same filled with vapour	0·042 gram.
Volume of balloon	=269·5 centim. cubic.
Residual air..........................	12·5 cub. cent.
Temperature of vapour	97·75 cent.
Temperature of air	17·0 cent.
Barometer = 0·744 metre.	

From these data follow:

Weight of a litre of vapour at temp. 0 cent. and pressure 0·76	= 1·4563 gramme.
Density of vapour (air = 1)......	1·1210
The theoretical density is	1·1105
The density found by Dumas ...	1·1200

The results of Dumas and Peligot may, therefore, be considered as completely established.

The formation of the first hydrate of methylene (the methylene æther) is the most important fact in the whole of Dumas's researches, as upon the idea of its basic reaction rests the similarity of the methylene with the alcohol series. The mode of analysis used by the French chemists consisted in the detonation of the gas with oxygen: in the repetition of the experiment I substituted a process in which the relation between the quantities was expressed by weight, a method less liable to error than that of measure.

The gaseous methylic æther was dissolved in water, which

was obtained quite free from atmospheric air by having been boiled for a long time, and then allowed to cool in closely stoppered vessels. A quantity of this solution was placed in a small retort, with which was connected a long tube containing fragments of chloride of calcium, to the other extremity of which was attached a tube containing black oxide of copper, and standing in connexion with the ordinary apparatus of Liebig for absorbing the products of the combustion. The tube with oxide of copper having been carried to a dull red heat, the retort was warmed until it began to give out pretty copiously the gaseous methylic æther, which streaming over the red hot oxide of copper gave water and carbonic acid. The process was continued until there were obtained:

Water generated by the combustion	=	1·286 gramme.
Carbonic acid		2·071 ———

giving

$$\frac{\text{Hydrogen}}{\text{Carbon}} = \frac{1411}{5726} = \frac{1000}{4058}. \quad \text{Theory } \frac{1000}{4083} = \frac{6\ \text{H}}{2\ \text{C}}.$$

The ratio of the carbon and hydrogen is the same as in alcohol, and as given by Dumas and Peligot; and as the atomic weight has been accurately determined by the analysis of the sulpho-methylates by Dumas and by myself, the basis of the methylene series is put beyond the possibility of doubt.

I have considered this confirmation of Dumas's view of the methylene alcohol and æther sufficiently important to be brought forward, and shall now pass to the examination of some results to which the French chemists did not attend.

Of the Compound of Chloride of Calcium with Pyroxylic Spirit.—This body, to which allusion has been already made, crystallizes readily in large six-sided tables (rhomboidal): exposed to the air it rapidly deliquesces, absorbing water, and pyroxylic spirit becoming free, but over sulphuric acid the crystals can be obtained completely dry. The analysis was made simply by rapidly weighing a portion in a platinum crucible, heating it gradually until all pyroxylic spirit was driven off, and then fusing and weighing the residual chloride of calcium. The result was as follows:

Pyroxylic spirit... =	3·168 :	53·3,	or 2 atoms =	53·565
Chloride of calcium	2·776 :	46·7,	or 1 atom	46·435
Grammes	5·944	100·0		100·000

It therefore corresponds to the alcoholates [alcoates] examined by Graham*.

Of the Oil which accompanies Pyroxylic Spirit.—In the

* [Mr. Graham's paper on the Alcoates was reprinted in Phil. Mag. and Annals, vol. iv. p. 265.—Edit.]

rectification of the different quantities of pyroxylic spirit used in the foregoing and subsequent experiments, I had an opportunity of collecting a certain quantity of that oil, to the presence of which I have had already occasion to allude. The quantity, although very small, allowed the determination of some of its most remarkable characters and of its composition.

This oil is colourless when first procured; by exposure to the air and to light it becomes coloured. Its odour is aromatic and resinous. Although, as we have seen, readily passing over in vapour with pyroxylic spirit at the temperature given by a water-bath, yet alone its boiling-point is very high. The quantity in my possession was, however, too small to allow of my determining it exactly. This oil is very light; it floats not only on water, but on every mixture of pyroxylic spirit and water by which it is not dissolved. The oil, having been purified from spirit by distillation, and from water by contact with chloride of calcium, gave the following analytical results:

From 0·388 material were obtained,

Water = 0·377
Carbonic acid = 1·176;

from whence follows the composition:

Carbon	= 83·78	or,	20 atoms carb.	= 84·18
Hydrogen	10·79		30 atoms hyd.	10·32
Oxygen	5·43		1 atom oxy.	5·50
	100·00			100·00

A second analysis was made a few days afterwards with the same oil, but which had in the interim become coloured by the action of the air.

0·335 material gave

Water 0·316
Carbonic acid 0·991;

from whence follows:

Carbon	= 81·73	or,	20 atoms carb.	= 81·93
Hydrogen	10·33		30 atoms hyd.	10·05
Oxygen	7·94		$1\frac{1}{2}$ atom oxy.	8·02
	100·00			100·00

It thus appears that in the interim the oil had absorbed half an atom of oxygen from the air.

The formula ($C_{20}H_{30}O$) is the same with that obtained by Fremy for resinain, from which body this oil is distinguished by its physical properties. The two substances are isomeric, but as none of the combinations have been examined for either, nothing can be said as to the relation between their atomic weights. This oil absorbs chlorine with considerable

rapidity, there is much heat disengaged, and a large quantity of muriatic acid gas formed, together with a dark brown heavy liquid, the detailed examination of which was prevented by the smallness of the quantity of oil in my possession.

Action of Chlorine on Pyroxylic Spirit.—In the memoir of Dumas and Peligot these distinguished chemists touch upon the action of chlorine on pyroxylic spirit but slightly; and I have found in my experiments a remarkable deviation from their account, so far at least as the accompanying phænomena are concerned.

In order to examine the nature of this reaction I employed in the first instance the apparatus used by Professor Liebig in the formation of chloral, but was obliged to abandon its use from the great violence of the action that ensued. Every bubble of dry chlorine that came into contact with the pyroxylic spirit produced an explosion, with flame, the deposition of carbon, and separation of muriatic acid; and if a few bubbles passed through the spirit without being absorbed and mixed with the vapour in the upper portion of the apparatus, a still more violent explosion took place, which generally drove the mass back into the vessel containing the sulphuric acid by which the chlorine had been dried. As light could not be completely excluded from acting on this apparatus, the following arrangement was substituted for it. The retort, in which chlorine was disengaged, was connected by a bent tube with a three-necked bottle containing sulphuric acid. In the second neck of this bottle was inserted a safety-tube dipping into the acid, and from the third issued a bent tube which passed through one opening of a two-necked balloon, and dipped into the pyroxylic spirit contained in it. With the second neck was connected the refrigerating apparatus of Liebig, which delivered the condensed fluid into a second two-necked globe, from which a tube conducted the disengaged muriatic acid gas to a solution of potash, in order to prevent the inconvenience of its escaping into the atmospheric air.

The balloon containing the pyroxylic spirit having been carefully covered with thick paper so as almost perfectly to exclude the light, the stream of dry chlorine was absorbed completely and much muriatic acid generated. After some time the action becomes less intense, and the balloon requires to be warmed to favour the escape of the muriatic acid formed. At the end of the reaction there are found in the balloon two liquids of different densities, one light, *watery*, and intensely acid, the other extremely dense, and generally coloured by carbon deposited in the explosions, a few of which are almost unavoidable. This heavy liquor possesses the following pro-

perties: it appears to be nearly as dense as sulphuric acid, tastes sour, reddens litmus-paper, probably from adhering muriatic acid, and had a very high boiling-point; with water it came over very readily, but alone it generally left a dark-coloured residue in the retort, and muriatic acid was set free.

Its composition was determined in the following manner: analysed with oxide of copper,

(No. 1.) 1·113 gramme material gave
Water ...... = 0·185
Carbonic acid = 0·890;

(No. 2.) 0·992 gramme material gave
Water ...... = 0·162
Carbonic acid = 0·787:

from whence results per cent.

	No. 1.	No. 2.
Carbon...... =	21·75	21·94
Hydrogen... =	1·73	1·81.

In these two analyses there came over with the current of carbonic acid gas and watery vapour a small quantity of subchloride of copper, which depositing itself with the water, renders the hydrogen estimate much too high. As in analyses of this kind this source of error is almost unavoidable, two analyses were made with oxide of lead, which, though producing but an imperfect combustion of the carbon, gave a result for hydrogen which I consider as nearly true. Thus with oxide of lead,

(No. 3.) 1·430 material gave 0·172 water, whence results 1·34 of hydrogen per cent;

(No. 4.) 1·136 material gave 0·143 water, whence results 1·39 of hydrogen per cent.

To determine the chlorine, the vapour of the body was decomposed by ignited lime; the mass of lime dissolved in diluted nitric acid, and the chlorine precipitated by nitrate of silver: in this way,

(No. 5.) 0·892 of material gave 2·416 chloride of silver, or 66·82 of chlorine per cent.;

(No. 6.) 0·544 of material gave 1·452 chloride of silver, or 66·0 per cent. of chlorine.

These results are represented with tolerable accuracy by the formula,

6 atoms carbon =	458·622	22·80
6 —— chlorine =	1327·950	66·17
4 —— hydrogen =	24·959	1·24
2 —— oxygen =	200·000	9·89
	2011·531	100·10.

This formula I do not bring forward as absolutely fixed, but as rendered extremely probable by the analyses detailed above, and by the reactions which are observed in the decomposition of this body by bases, a subject to which I shall recur in another series of these researches.

[To be continued.]

XIII. *On Professor* MÜLLER'S *Account of the Reflex Function of the Spinal Marrow*. Communicated by* MARSHALL HALL, *M.D., F.R.S., &c.*

SINCE the publication of my Memoir on the Reflex Function of the Medulla Oblongata and Medulla Spinalis, inserted in the Transactions of the Royal Society for 1833, I have been greatly gratified to find that Prof. Müller, the justly celebrated physiologist of Berlin, had been led, entirely independently of me, into the same path of investigation,—to nearly similar results,—and even to the adoption of the same designation † for the special function of the spinal marrow which is the subject of my inquiries.

Prof. Müller states, as will appear from the paragraph of which, by the kindness of Mr. Paget, I am enabled to send a translation, that the first part of his *Handbuch*, containing the principles of the reflex function, was published in the spring of 1833, the very year in which my paper was published in the Philosophical Transactions‡. I had, however, read a short account of the same principle of action in the spinal marrow, to the Zoological Society, the year previously, viz. 1832, which was published in the "Proceedings of the Committee," and referred to in the Lond. & Edinb. Phil. Mag., vol.ii. p. 477; so that the question of priority of publication is decidedly in my favour. At the same time, the almost perfect coincidence in our observations and experiments, and in our conclusions from them, is at once most remarkable and satisfactory. The name of Prof. Müller will not fail to give importance to the inquiry; and, for my part, I recall to mind, with pleasure, the remark of Sir Humphry Davy, that "we may generally discover how our labours will be appreciated eventually, from the opinion of contemporary foreigners, who being unbiassed by circumstances of personality, will reduce every object to its just proportions and value."

* *Handbuch der Physiologie.*

† Prof. Müller goes further. He says—"The spinal marrow has the property of reflecting sensorial impressions made upon the sentient nerves, to the motor nerves. *It is a reflector*," &c. (*Opus cit.*, p. 789.)

‡ [A notice of the reading of Dr. Hall's paper before the Royal Society was given in Lond. & Edinb. Phil. Mag., vol. iii. p. 460.—EDIT.]

"*Of the Reflexion in the Motions after Perceptions.*

"The observations which are brought forward in this Chapter are new, and denote a remarkable progress in our science. They relate to phænomena of the so-named sympathetic motions after perceptions, which were formerly very liberally supposed to be exercised by the sympathetic nerve, though it may be clearly proved that they take place quite independently of it. As the phænomena belonging to this class are uncommonly numerous, and include a great part of the phænomena which were formerly without any proof attributed to the sympathetic nerve, the use of the sympathetic nerve in the explanation of nervous sympathies seems constantly to diminish. How much this part of physiology has altered, is clearly seen, by comparing the explanation of a great part of the nervous sympathies which the admirable Tiedemann investigated in the year 1825*. The explanations of the sympathies by means of the sympathetic nerve, in fact, explain everything and yet nothing. Thus, as this work will fully show, the most evident and frequent sympathies between the uterus and mammæ, the parotid and testes, the larynx and testes, are quite inaccessible to these explanations. We will not positively say that the sympathetic nerve does not take a part in any of the sympathetic phænomena, but we do altogether deny that the sympathetic nerve participates in all the so-called sympathetic phænomena, which will be examined in this chapter, and we think it very probable that the sympathetic nerve is generally unconnected with the greatest part of those sympathies, in which motions take place after perceptions, or perceptions after other perceptions, or motions after motions. The explanation of sympathies by nervous connexions had been already made very questionable by the microscopic anatomy of their primitive filaments. For how could this explanation be received, when at present, though we know of connexions of the fasciculi of the nerves, we are acquainted with no union of their primitive filaments? A mere nervous connexion, therefore, without any ganglion on the part, cannot of itself in the present state of the science explain any sympathy.

"The phænomena now to be examined were observed at nearly the same time by Dr. Marshall Hall and myself. As the greatest part of the 'Nervenphysik,' as here given, was completely prepared several years since, so also this Chapter on the reflected motions after perceptions, was written down almost exactly as here given several years ago. That this

* *Zeitschrift für Physiologie*, i.

explanation is correct, is shown by the first part of this manual, which appeared in the spring of 1833, and which in pages 333—335* already developed the fundamental principles

* "The system of the respiratory nerves may be thrown into morbid action, producing convulsive motions, by local stimuli in all parts which are provided with mucous membranes. Stimuli applied to the mucous membrane of the nose, produce sneezing; in the pharynx, œsophagus, stomach, or intestines they produce the concurrence of the respiratory motions, in vomiting; while powerful stimuli in the rectum, urinary bladder, or uterus, produce a concurrence of respiratory motions in the involuntary discharge of fæces or urine, or the expulsion of the fœtus. Stimuli of the mucous membrane of the larynx, trachea, and lungs, nay, even a stimulus exciting a tickling in the Eustachian tube, produce cough.

"All these involuntary motions, cough, vomiting, spasmodic involuntary discharge of fæces, the forced passage of urine, are produced with the assistance of the respiratory motions. The local stimulus here acts from the inner membrane of the viscus, on the branches of the sympathetic ramifying therein, and in the stomach, pharynx, trachea, and lungs, on the branches of the vagus which they receive, or in the nose on the nasal branch of the trigeminus, and is reflected to the source of the respiratory motions in the medulla oblongata and to the spinal marrow, from which proceed the groups of respiratory motions that produce vomiting, cough, sneezing, &c. Stimuli of the nasal branches of the trigeminus produce sneezing, even when secondary; for instance, when the stimulus of the sun's light acts first on the optic nerves, the latter act on the brain, and the brain causes a secondary excitement of the nasal nerves and coincidently of the respiratory nerves. I, like many other persons, sneeze as soon as I see bright sunlight. Stimulus of the vagus alone in the larynx, trachea, and lungs excites cough, that of the pharyngeal branches of the vagus and glossopharyngeal in the pharynx, or of the vagus in the stomach, excites vomiting. We will now go through the several groups of these sympathetic respiratory motions.

"All the several respiratory motions may be produced in an isolated manner, and sometimes are united in groups, such as do not regularly occur in respiration.

"The contraction of the diaphragm, united with the motions of respiration, takes place, voluntarily or involuntarily, in the forcible expulsion of a body from parts of the abdominal cavity; e. g. voluntarily in the expulsion of fæces and of urine, involuntarily in vomiting, parturition, involuntary expulsion of fæces after their too long retention, and in the involuntary discharge of urine long retained. The pharynx, stomach, rectum, urinary bladder, and uterus, all stand by means of their nerves in such connexion with the cerebral and spinal nerves, that a violent stimulus applied to any one of them, excites contraction not merely in it, but also in the abdominal muscles and diaphragm, to the expulsion of the irritating matter upwards or downwards. This effect takes place by reflexion to the brain of the stimulus of the branches of the vagus in the pharynx and stomach,—to the sympathetic system and to the brain and spinal marrow, from the sympathetic twigs of the stomach,—and by the reflexion to the spinal marrow of the stimulus of the partly sympathetic and partly sacral nerves in the rectum, uterus, and urinary bladder. In all these motions for the expulsion of a part upwards or downwards, the glottis is for a long time closed.

"For the explanation of the production of vomiting, an observation of mine is very instructive, viz. that if we open the cavity of the abdomen in a rabbit, and having exposed the splanchnic nerve on the left side (near the

of the reflected motions after perceptions, from observations which will be here further detailed. It is remarkable that

inner side of the renal capsule) tear it with a needle, contraction of the abdominal muscles often takes place. I have not seen this in the dog.

"In cough, the stimulus of the vagus, in the larynx, trachea, and lungs is propagated to the medulla oblongata. The medulla oblongata thereupon excites contraction of the glottis, with spasmodic expiratory motions of the thoracic and abdominal muscles, by which at each expiratory action, the previously closed glottis is somewhat opened and a loud tone produced. The diaphragm has nothing to do with the cough, except that sometimes a deep inspiration is made before coughing. According to Krimer[1] and Brachet, after division of the vagus on both sides of the neck of an animal, cough can no longer be excited by violent stimuli of the internal surface of the trachea. It certainly, however, may, according to Krimer, after division of the sympathetic nerve in the neck.

"We have the power of closing the entrance into the larynx, not merely by the closure of the glottis, but even in the fauces from the nasal and oral canals. Dzondi discovered that this takes place by the approximation of the posterior arches of the palate, which lie almost like two curtains approaching each other from the sides, and by the apposition of the posterior part of the tongue against this inclined plane. This motion always precedes sneezing.

"Sneezing is a violent sudden contraction of the expiratory muscles, after the air-passages anteriorly have been previously closed. This closure changes at the moment of the violent expiration into a sudden opening of the oral and nasal canals together, or of the latter alone. Sneezing has nothing whatever to do with the diaphragm, which so many ancient and modern authors have supposed to take a part in it. The widely spreading nervous sympathies appear quite unnecessary in the explanation of sneezing. In the false supposition that sneezing is effected by the diaphragm, it was thought that the stimulus of the nasal nerves was propagated to the deep twig of the Vidian, and to the sympathetic, and from thence to the cervical and the phrenic nerves. Even Arnold still speaks of this. Now as the expiratory muscles (with previous closure of the mouth and nose) produce the act of sneezing, and not the diaphragm, the simplest view is to regard the medulla oblongata itself as the medium between the nasal branches of the trigeminus, the expiratory muscles, and the muscles of the velum palati, after the analogy of the sympathetic motion of the iris by the stimulus of light. For in this case, as may be clearly shown, the stimulus of light acts neither immediately on the ciliary nerves, nor from the retina to the ciliary nerves. The arteria centralis is indeed, according to Tiedemann's discovery, accompanied by a fine twig from the ciliary ganglion. But this twig is distributed on the arteria centralis retinæ, and is in no proved connexion with the retina. In complete paralysis of the retina, light in general no longer produces contraction of the iris, though still through the healthy eye it does produce a contraction of the iris of the diseased one. (There are however exceptions to this rule, which Tiedemann has collected in his *Zeitschrift für Physiologie.*) The motion of the iris therefore probably results from a reflexion of the stimulus of the retina to the brain, from the brain back to the oculo-motor nerve, and the ciliary ganglion. The sympathies of a great part of the nerves with a local stimulus through the medium of the brain and spinal marrow, are very well shown in the phænomena following the narcotization of an animal, in which a slight touch on the skin produces general tetanic spasms."—pp. 333—335.

[1] *Untersuchungen über den Husten.*

the same ideas, with the same instances and observations on narcotized animals, were propounded in the same year by Dr. Marshall Hall, in the Philosophical Transactions of 1833. Although these ideas were formed by us independently of each other, yet the great correspondence in the observations and explanations is not difficult to account for, if one considers, that the physiology of the nerves has attained a condition, such that in pursuing the subject the most remote observers may at the same time be led to similar new observations and explanations. I shall in the following pages first communicate my own observations, as they were originally formed, and shall then compare them with the results of the English physician and physiologist.

"When perceptions, which are produced by external stimuli on sensitive nerves, produce motions in other parts, this never takes place by a reciprocal action of the sensitive and motor filaments of the nerves, but by the sensorial excitement acting on the brain and spinal marrow, and from these back to the motor filaments. This extremely important principle in physiology and pathology requires a strong proof, which may be very clearly attained empirically, and then explains a number of physiological and pathological phænomena.

"I shall first prove that the motor and sensitive filaments of a nerve, after the connexion of their two roots, do not enter into any connexion with one another, but run separately to their respective parts, and that therefore, even in cases where the nervous sympathy is not in play, the sensitive and motor filaments of a nerve have no reciprocal action whatever.

"The proof of this position may be shown clearly in the following manner: If a compound nerve be stimulated (after being divided,) at its central portion, severe pain is produced, and the animal may express this pain by motions of flight, crying, &c.; but the muscular nerves connected with the stimulated portion are not excited to action. No twitchings take place in the muscles which receive nerves from the divided trunk.

"It may also be proved in the following way: As the three nerves destined for the posterior extremity of the frog form a plexus, which again gives off two nerves, so, if one of the latter nerves be divided and isolated from all its connexions with the muscles, and then the central portion be mechanically stimulated, the injury produces a centripetal excitement of the sensitive fibres of this nerve; but the other nerves, proceeding from the same plexus, do not, when the isolated nerve is injured, excite any twitchings in their muscles. That moreover, the general twitchings that ensue on any

touch, in narcotized frogs and other animals, are only produced through the medium of the spinal marrow and brain, may be decisively proved; for if a limb be cut off from a narcotized frog, touching it (the limb) will not produce twitchings in it. These experiments are still more instructive in the lizard.

"The spotted lizard retains for a long time after division of the spinal marrow, the so-named sensitive power in all parts below the section; or, if this cannot be called sensitive power, the capability of propagating sensitive impressions to the spinal marrow, and of re-acting by twitchings. Even the end of the tail has still perception; nay, this power is as much elevated by the division of the spinal marrow, as in frogs which have been previously narcotized: if a portion of its trunk after being cut off be only very lightly touched, it always contracts, and this continues for hours. But this interesting phænomenon is only shown when the spinal marrow is still contained in the separated piece, and not in whole limbs separated from the trunk and not containing any spinal marrow. These interesting facts I observed several years ago, 1830, when with Herr Jordan I was investigating the poison of the cutaneous glands in the spotted lizard. It results from this that the general twitchings which take place in animals on touching particular parts, do not result from communication of sensorial and motor nervous filaments, but that the spinal marrow is the connecting medium between the sensorial-centripetal and the motor-centrifugal excitement.

"The phænomenon of general twitchings after local perceptions is therefore also independent of the sympathetic nerve, and is induced by an irritation of the spinal marrow, by which every purely local sensorial-centripetal excitement propagates itself to the whole spinal marrow and brain, and from thence of necessity excites all motor fibres. But this irritable condition is excited by the following causes:

"1. In many animals by the mere division and injury of the spinal marrow. Thus tortoises move after the head is cut off, whenever they are touched; and young birds move on being touched immediately after decapitation, as do also all parts of the cut-off trunk in the lizard.

"2. Further, the spinal marrow is irritated to this degree in the first stage of narcotic poisoning in frogs, as well as in mammalia, which move after poisoning with nux vomica, whenever they are touched. This stage of excitable debility in narcotization almost always precedes the stage of paralytic debility.

"3. Other causes also, which debilitate the brain and spinal

marrow by stimulation, produce the same phænomenon. In men with excitable debility of the nervous system any unforeseen sensation, sound, touch, or mechanical shock produces a general start. So also in men who, by stimulation of the genitals, and thereby of the spinal marrow, or by other causes, have acquired an excitable debility of the spinal marrow. We may here cast a glance at the nature of nervous irritation. All nervous stimuli may induce in succession three conditions. First, excitement, in which the powers appear still uninjured. Second, in proportion as the excitement is repeated, excitable debility. Third, atonic debility.

"4. A local violent excitation of a sensitive nerve, may by the violence of the centripetal excitation of the brain and spinal marrow, induce twitchings and tremblings, as after a severe local burn, in tooth-drawing, &c.

"5. Local stimulations of the nerves by inflammation or tumours, often also produce general spasms, or even epilepsy.

"6. The irritation of the spinal marrow, originating from local sensorial excitement, may in violent injuries be so great, that the movements are constant, and continue even without touching. This irritation of the spinal marrow resulting from severe local nervous injuries, is the tetanus traumaticus. Every severe irritation of the spinal marrow generally is tetanus, whether produced by narcotic poisoning or locally and indirectly. I have here shown how the production of tetanus traumaticus is to be explained by simple empirically determined facts.

"7. The severe irritation of the sympathetic nerves of the intestinal canal also excites by acting back on the central parts general spasms; and thus the cramps in sporadic cholera, as well as the convulsions in the diseases of the viscera in children, are to be explained.

[To be continued.]

XIV. *Reply to* Mr. Rainey's *Communication in the Phil. Mag. for December* 1836. *By the Rev.* William Ritchie, *LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution of Great Britain and in the University of London.*

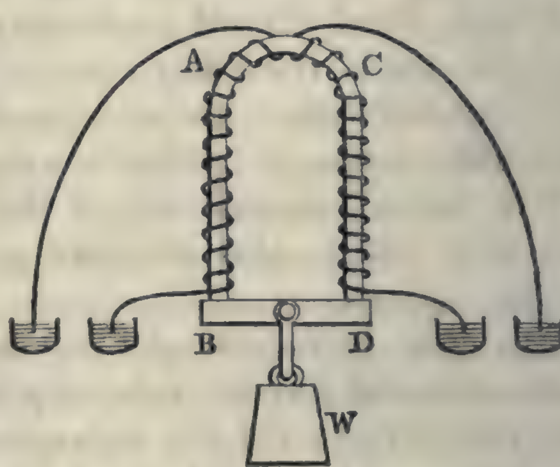
To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

It is not with any wish of having the *last* word in the discussion of the *principles* at issue between Mr. Rainey and myself, but simply with a desire of establishing scientific truth on a

firm basis, that I again trouble you with the following remarks.

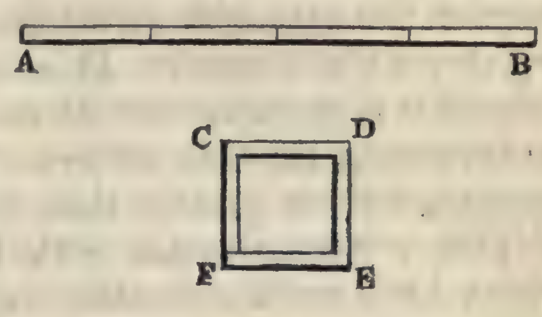
1st. Mr. Rainey's first false position is, that "there is no known limit at which soft iron ceases to allow of further induction by an increase of the inducing power," and he also takes for granted, that a magnet having *double* the power will induce on the *same* armature twice the effect. The following experiments must convince every person that the lifter or armature has a *limit* to its capability of receiving magnetism, and that its capability of receiving new increments *diminishes* rapidly as the inducing power increases.



Roll a covered wire about the half of an electro-magnet from A to B. Do the same with an equal wire from C to D. Connect the first helix with an elementary battery, and ascertain the lifting power of the magnet. Connect the other helix with an *equal* battery, and instead of the lifting power being *doubled*, acccording to the principle assumed by Mr. Rainey, its power may not be increased a third or fourth, or even a tenth, if the battery be a powerful one.

2nd. The second *false* principle which Mr. Rainey assumes is, that the lifter, by its *reaction*, can induce a higher state of magnetism in the *inducing* magnet than what it naturally possesses. The experiment from which he deduces this novel principle by no means warrants the conclusion. The experiment every person who knows anything of the subject, knows to be correct; the reasoning by which the conclusion is deduced must be as easily seen to be fallacious. The experiment is, that a soft steel electro-magnet receives more powerful *permanent* magnetism when a piece of soft iron is across its poles than it does when the poles are not united. Mr. R. concludes that "as the induction from the *galvanic* current was the same in each stage of this experiment, the increase of permanent magnetism would appear to depend upon the reaction of the keeper, which, by the converse of Dr. Ritchie's reasoning, must then possess a higher state of magnetism than the magnet to which it is applied." Here again Mr. Rainey takes for granted that the induction from the galvanic current is the same in the steel magnet, whether it form a *closed* circuit, as it does when its poles are connected by soft iron, or an *interrupted* circuit, when the poles are not connected. Now every person who

has magnetized a closed circuit knows that this is *not* the case. The following experiment will set this point at rest: take a bar of steel, A B, and bend another of the same size and length into a square. Magnetize the straight bar by drawing it lengthwise over one of the poles of a magnet; move the same pole the same number of times round the square;



break the bar into four equal parts and the square at the corners, and the bar C D will be stronger than either portion of the straight bar. I formerly stated, and again repeat the affirmation, that if Mr. Rainey's explanation be admitted, it will completely overthrow the Newtonian law of the perfect equality of action and reaction. "I had always considered," continues Mr. Rainey, "that law as applicable only to mechanical forces, and not extending in the least to those physical phænomena, the acting cause of which is altogether unknown. Suppose a number of pieces of steel, properly tempered, and for convenience made into the form of the common horseshoe magnet, and one of them magnetized to saturation; now, by this one let all the others be magnetized, and afterwards let them be put together, and the process of magnetizing be performed repeatedly upon each of the rest, and it will be found that each magnet possesses nearly, if not quite as much magnetism as the one employed in the first instance, that is, as the prime motor itself. This fact can scarcely be doubted, although it is at variance with the Newtonian law of the perfect equality of action and reaction as applied to mechanical forces, as no force can be supposed capable of generating, under the same circumstances, a force greater than itself." It is but too obvious from this quotation that Mr. Rainey has not read the *Principia* with any degree of attention, as he has entirely mistaken the meaning of the law in question. The acting cause of *universal attraction* is as much unknown as that of any other physical phænomena, yet the law holds as much in this case as in the case of a man *pulling* a boat against a stream. The second part of the preceding paragraph is exceedingly unfortunate as an illustration of a fact at variance with the Newtonian law. Nothing could be more conclusive in its favour. The following illustration would be equally applicable: suppose a number of pieces of wood placed at the mouth of a river, and suppose they are of such a size, that a man, after fixing a rope to one of them, is just able to pull it a mile against the stream and fix it to a tree growing by the side of the river; and suppose that he

again pulls another to the same place and secures it in like manner, and so on till he has secured the whole raft, and it will be found that if there be a hundred logs of wood, it will require the strength of a hundred men to prevent their floating down the stream. This fact can scarcely be doubted, although it is at variance with the Newtonian law, as no force can be supposed capable of generating, under the same circumstances, a force greater than itself.

This communication will, I trust, end a controversy from which little more scientific truth can be elicited.

XV. *Remarks on two of the Electric and Magnetic Communications in the last Number of the Phil. Mag. By* the Rev. WILLIAM RITCHIE, *LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution of Great Britain and in the University of London.**

ON no branch of science have there been so *many writers* and so *few* readers as on that of electricity and magnetism. This is fully illustrated by all the papers on that subject in the last Number of the Phil. Mag. (vol. ix. pp. 452, 469, 472.) The few remarks which I made on Mr. MacGauley's paper, read at the last meeting of the British Association, were regarded by him as bitter and uncalled-for, though those remarks related only to want of originality of his communication, as the facts had all appeared in print before that period. His last paper in the Phil. Mag. I consider exactly in the same predicament. If papers of this kind pass without animadversion, their authors will, at least by the uninitiated, be regarded as the real discoverers of the facts and reasonings contained in them. I feel that in undertaking the refutation of alleged claims I am undertaking an unpleasant and invidious task. Justice demands it at the hands of some one, and the person best able to do it is mainly concerned; he has perhaps wisely left it to others.

The first position claimed by Mr. MacGauley will be found in a paper of mine in the Phil. Mag. for last June, vol. viii. page 458. His second position is contained in Dr. Faraday's papers on the *length* of the coil influencing the spark, and the mutual action of the spires of the helices. Mr. MacGauley in his third position arrives at the strange conclusion that "magnetism within a helix proportionably injures its effect." Dr. Faraday has clearly shown that it is only magnetism in *motion* that induces electricity. If the soft iron be within a coil which has magnetism induced on it, at the same time that the coil has *voltaic* electricity induced on it, and if the temporary magnet in returning

* Communicated by the Author.

to its neutral state tend to induce electricity on the coil in the *same direction* with the returning current in the coil, the *effect* will be *increased*; on the contrary, *diminished*, which seems to be the case which presented itself to Mr. MacGauley's observation. His fourth position has been known so long that I scarcely know its discoverer. Sir H. Davy knew it well, and endeavoured to investigate the law of diminution in the effect of the battery depending on the length of the wire connecting its poles. The only thing *new* in this paper is the statement of a fact which is *not* correct. It is said that if *two* persons put their hands in salt water, and if they are placed in the electric circuit, *each* receives a more powerful *shock* than when only one person was placed in the circuit. That this is *not* the case every one who possesses a magneto-electric machine may easily satisfy himself.

The paper in page 472, by the Rev. N. T. Callan, is equally destitute of originality. The battery which he describes has been known for a long time. I had one exactly the same, made by Mr. Newman six or seven years ago, and frequently used at my public lectures in the Royal Institution. The author talks of the "enormous quantity of electricity circulated by this battery." The quantity is simply, as I have shown in the Philosophical Transactions, directly as the surface, and when used as a compound battery, as the square root of the number of plates. The author speaks of a *shock* as if it were a *quantity*, and institutes a comparison between the *size* of the shock and the *number* of plates. "When two pair of plates were used the shock appeared to be doubled; with *three* voltaic circles it appeared to be trebled." This, I believe, is the first attempt to bring *sensations* under the power of *analysis*. It is needless to dwell on this paper. The *only* thing new in it is the affirmation that an "electro-magnet, when its magnetism is induced by a *compound* battery of 200 small pairs of plates, will have a greater power of inducing magnetism at a distance than any permanent magnet." The very looseness of this statement is a proof of its fallacy. Does the author mean to say that a *small* electro-magnet when connected with a battery of 200 pairs of plates induces more magnetism on soft iron at a distance than *any* permanent magnet? Though he says so he cannot seriously mean what he says. His meaning then must be this, that if an electro- horseshoe-magnet have the same *lifting* power with a permanent one when the keeper is in contact with it, it will lift a small piece of soft iron at a greater distance than the electro magnet. That this is *not* the case every person may satisfy himself by the simplest experiment. I have shown it to be so in the Phil. Mag. for last August, (vol. ix.

p. 81,) which the author seems to have glanced at but not read. In that paper I stated that "the attraction of an electro-magnet for pieces of soft iron *at a distance* was much less than that of a permanent magnet of equal lifting power. This peculiar property rendered the electro-magnet not well suited for magnetic induction at a distance; and hence after a few unsuccessful trials to substitute it for the permanent magnet in my apparatus for continued rotation, it was long since abandoned." It was abandoned for this reason, not because it failed in producing rotation, but simply because it did not do it so well as a permanent magnet. The author in question gives the following version of my statement: "Dr. Ritchie says that the use of the electro-magnet in the apparatus for continued rotation was long since abandoned, because it was incapable of inducing magnetism in an iron bar at a distance." It is painful to be forced to notice papers of this kind with which the journals are constantly filled. It may appear to some that my assertions are too broad. I appeal for the truth of them to every person acquainted with the present state of the science.

XVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. ix. p. 537.]

June 16. 12. "A Comparison of the late Imperial Standard Troy (*Continued.*) Pound Weight with a Platina copy of the same, and with other Standards of authority." Communicated by Professor Schumacher, in a Letter to Francis Baily, Esq., V.P. and Treas. of the Society.

Professor Schumacher being desirous of procuring an accurate copy of the English Imperial Standard Troy pound weight, for the purpose of comparison with the Danish weights, applied to Capt. Kater, requesting him to cause such copy to be made; which was accordingly done. It was made of brass by Bate; but the result of the weighings not being satisfactory to Professor Schumacher, he desired to have a second copy forwarded to him. As these two copies did not agree in their results, the first was returned to Capt. Kater with a request that he would repeat the weighings. The result confirmed Professor Schumacher's suspicions: and as it was not thought proper that, in an affair of so much importance as the comparison of the standard weights of two nations, any source of discordance should exist, or even be suspected, (the preceding experiments having been made with a *copy* of the Imperial standard weight) the Danish Government sent over Capt. Nehus (of the Royal Danish Engineers) to this country for the express purpose of making comparisons with the *original* standard, in the possession of the Clerk of the House of Commons.

The weighings took place in the Apartments of this Society, and were partly made with Ramsden's balance, belonging to the Society. Besides the first brass weight above mentioned, there was another brass weight made by Robinson, a platina weight made by Cary, the brass pound weight belonging to the Royal Mint, and the platina pound weight belonging to this Society. These were all subjected to a most rigid and accurate series of weighings by Capt. Nehus, in which every precaution was taken to insure the most correct results. It would be impossible here to follow Capt. Nehus through all his details: but it may be sufficient now to state that upwards of 600 comparisons were made with the English Imperial standard, all of which are apparently very accordant; but, on account of a singular circumstance connected with the *original* standard, do not possess that degree of precision, nor afford that satisfaction which ought to attach to an affair of so much importance. For, it appears that not only the specific gravity of the original standard had never been ascertained, but that we are even ignorant of the kind of metal of which it was composed: some persons maintaining that it was of brass, others of copper, and others of bell-metal. And, as the original was totally destroyed in the late fire which consumed the two Houses of Parliament, we cannot now supply this omission. It is well known that the specific gravity of brass may vary from 7·5 to 8·5; so that a difference of at least $\frac{1}{8}$ of a grain might arise from this circumstance alone; setting aside a number of other particulars that require minute attention, and which do not seem to have been attended to in former experiments of this kind. In fact, as Professor Schumacher remarks, though we have thus five different pounds in excellent preservation, and compared with the lost standard, with the greatest care and the best instruments, though the number of these comparisons exceeds 600, yet there still remains an uncertainty as to its real weight; and this solely on account of its specific gravity and expansion not being known. And, he adds, that it is to be hoped that no pound will in future be declared a legal standard unless these elements (the knowledge of which is indispensable even for a single comparison with a good balance) are previously determined with the greatest possible precision.

Besides the account of these numerous weighings, which are stated in detail, Professor Schumacher has given various formulæ and tables which will be found of great use and application in any future experiments of a like kind that may be undertaken.

13. "On the Application of a New Principle in the Construction of Voltaic Batteries, by means of which an equally powerful current may be sustained for any period required; with a description of a sustaining battery recently exhibited at the Royal Institution." By Frederick W. Mullins, Esq., M.P., F.S.S. Communicated by N. A. Vigors, Esq., F.R.S.*

The method resorted to by the Author for obtaining a continuous voltaic current of equal intensity, is the same in principle as

* See Lond. and Edinb. Phil. Mag., vol. ix. pp. 121, 283.

the one employed by Professor Daniell, and described by him in his paper recently presented to the Royal Society, and published in the Philosophical Transactions; namely, the interposition of a thin membrane between the two metals in the voltaic circuit, so as to allow of the separation of the different fluids applied respectively to each metal: the fluid in contact with the zinc being a mixture of diluted sulphuric and nitric acids; and that in contact with the copper being a solution of sulphate of copper. The author reserves for a future paper the details of the results he has obtained, with regard to the relations between the intensity of effect, and the extent and disposition of the metallic surfaces: but states that he has obtained powerful electric action by bringing the membrane into contact with the zinc; the latter having no acid applied to it, and the only fluid employed being the solution of sulphate of copper.

14. Anonymous Essay, entitled "Scoperta della Causa Fisica del Moto." Presented to the Royal Society, with a view to obtaining one of the Royal Medals for 1836.

The Author commences by an historical review of the opinions of almost every philosopher, both ancient and modern, who has treated of the subject of motion, from Pythagoras to Le Sage: and proceeds to state his own ideas relating to the cause of motion, founded on the hypothesis that the ultimate atoms of all matter have a pyramidal figure.

15. "An Experimental Inquiry into the Modes of Warming and Ventilating Apartments." By Andrew Ure, M.D., F.R.S.

The Author, having been consulted by the Directors of the Customs Fund of Life Assurance, on the mode of ventilating the Long Room in the Custom House, and deeming the subject one of great public interest, was induced to lay the result of his observations and experimental inquiries before the Royal Society. In this room, about two hundred persons are busily engaged in transacting the business of the Institution. All these persons are found to suffer more or less from ailments of the same general character, the leading symptoms of which are a sense of fulness and tension in the head, flushing of the face, throbbing of the temples, giddiness, and occasional confusion of ideas, depriving them of the power of discharging their duties, in which important and frequently intricate calculations are required to be gone through. These symptoms of determination of blood to the head are generally accompanied by coldness and languid circulation in the feet and legs, and by a feeble, and frequent, as well as quick and irritable pulse. On examining the air of the room by appropriate instruments, the author notices more especially three circumstances in which it differs from the external air: first, its temperature, which is maintained with great uniformity within a range of 62° to 64°; secondly, its extreme dryness, which, on one occasion, measured by Daniell's hygrometer, was 70 per cent.: and thirdly, its negatively electrical state, as indicated by the condensing gold-leaf electrometer. In all these qualities the air respired by the inmates of the room bears a close resemblance to the pestilential blasts of wind which, having passed

rapidly over the scorching deserts of Arabia and Africa, constitute the *Simoom* of those regions, and are well known by their injurious effects on animal and vegetable life. To these noxious qualities is superadded, as in the air of all rooms heated through the medium of cast-iron pipes or stoves, an offensive smell, arising partly from the partial combustion of animal and vegetable matters always floating in the atmosphere of a town, and perhaps also from minute impregnations of carbon, sulphur, phosphorus, or even arsenic, derived from the metal itself. The Author expresses his surprise that in the recent report of the Parliamentary Committee on the subject of ventilation, no reference is made to the methods employed for that object in factories, although they afford the best models for imitation, being the results of innumerable experiments made on a magnificent scale, with all the lights of science, and all the resources of the ablest engineers. He proceeds to describe these methods; and is then led to investigate the comparative efficiency, with a view to ventilation, of a draught of air resulting from a fire and chimney, and that produced by the rotation of a fan-ventilator. He shows that a given quantity of coal employed to impart motion to the latter, by means of a steam-engine, produces a ventilating effect 38 times greater than can be obtained by the consumption of the same fuel in the ordinary mode of chimney ventilation. Accordingly, he strongly advises the adoption of the former in preference to the latter: and inveighs against the stove-doctors of the present day, who, on pretence of economy and convenience, recommend the slow combustion of a large body of coke, by means of a slow circulation of air; under which circumstances, it is well known to chemists that much carbonic oxide, a gas highly pernicious to all who respire it, is generated; accompanied, at the same time, by a comparatively small evolution of heat. In order to obtain the maximum quantity of heat from a given mass of fuel, its combustion, he observes, should be very vivid, and the evolved caloric should be diffused over the largest possible surface of conducting materials; a principle which has been judiciously applied in several French factories. It has been proved that work-people employed in calico-drying rooms, heated according to the plan here reprobated, become wan, emaciated, and diseased; while in rooms in which the air is more highly heated by means of steam-pipes, they preserve their health and florid complexion.

16. "An Experimental Inquiry into the Relative Merits of Magnetic Electrical Machines and Voltaic Batteries, as Implements of Philosophical Research." By William Sturgeon, Esq., Lecturer on Natural and Experimental Philosophy at the Honourable East India Company's Military Academy at Addiscombe. Communicated by P. M. Roget, M.D., Sec. R.S.

The first part of this paper is occupied by a description of two forms of constructing the magnetic electrical machine, which the author has adopted; and the second, with the particulars of some experiments made with a view to determine the respective powers of these machines as compared with the common voltaic battery. In the first form of the instrument, a reel, round the periphery of

which 200 feet of copper wire, one 20th of an inch in diameter and covered with stout sewing-silk, are coiled, is made to revolve on a spindle, placed in the axis of a system of horse-shoe magnets, so as to remain within the branches of the latter during its whole revolution. The electric currents produced in the copper wire by magnetic induction, while the coil is moved at right angles to the plane of the magnets, are conducted by means of four semicircular metallic flanges attached to the spindle, into cisterns of mercury, the one being positive, and the other negative; and which consequently act as the two poles of the battery. In the second form of the apparatus, a piece of soft iron, of which the ends are bent into the shape of two arms, and which is surrounded with a coil of 300 feet of copper wire, is made to revolve in front of the poles of a horse-shoe magnet; its axis of motion coinciding with that of the magnet; and the electrical currents determined in the wire by this rotation, being collected in the same manner as in the former instrument.

The author next details several series of experiments which he made for the purpose of ascertaining the relation observable between different velocities of rotation in these instruments and the corresponding effects: first, with regard to the deflection of a magnetic galvanometer; secondly, with regard to chemical decompositions; thirdly, with regard to the production of sparks; and lastly, with regard to the intensity of the shock communicated to the human body. He compares the effects produced by the magnetic electrical battery, first, when the coil consisted of one continuous length of wire; secondly, when the coil was doubled upon itself so as to constitute two sets of conductors of half the length of the former; thirdly, when, upon being again doubled, it composed four conductors of one quarter of the length of the first; and lastly, when, on being doubled a third time, the electric current was made to pass through eight wires, each one eighth of the original length of the single wire. It was found that by thus multiplying the channels of conduction, although both the magnetic and the luminous effects continue to be produced with scarcely any sensible difference of intensity, the power of effecting chemical decompositions becomes more and more impaired, and the physiological influence is weakened in a still more remarkable degree. In the four-stranded coil, indeed, no shock whatever could be produced, however rapidly the instrument was made to revolve. The author endeavours to account for these variations of effect by the diminution of velocity in the electric current, its quantity remaining unaltered, consequent on its division into several streams by the multiplied channels offered to its progress. He also tried the effects of conjoining the magnetic electrical machine with ordinary voltaic combinations; sometimes acting in cooperation, and at other times in opposition to one another; and notices the corresponding results, which were sufficiently accordant with theory.

17. "Welt Mechanik." By M. Kropalschek.

The object which the author has in view, in this paper, is to overturn the theory of universal gravitation, as regulating the planetary motions. The memoir is divided into two parts; in the first, he dis-

putes the accuracy of Kepler's law respecting the description of equal areas in equal times, and endeavours to confute the fundamental doctrines of astronomy relating to the elliptical orbit of the earth, the difference between solar and mean time, and the whole theory of the motions of the moon and the planets. In the second part, the author enters into a detailed exposition of his own views of the mechanism of the heavens; and devotes 215 closely written pages to the development of a perfectly new hypothesis, which he advances, founded on a supposed variation of the progressive motion of the planets, in an orbit perfectly circular, and by which he thinks he can explain all the phænomena they present to observation.

18. "Plan et Esai d'un nouveau Catalogue Sidéral, avec une répré-sentation graphique, et une loi de simple et régulière distribution des étoiles autour du Pole, qui pourra fournir plusieurs avantages à l'Astronomie pratique." By Professor Joseph Bianchi, Superintendent of the Observatory at Modena.

The Author proposes the construction of a new sidereal catalogue, accompanied with a graphic representation of all the stars visible within the field of view at each observation, by means of the meridian transit of the most conspicuous stars across the field of a telescope of four inches aperture, attached to a three-feet circle. He directs this telescope to any elevation of the heavens that happens to be clear; and bringing any conspicuous star to the horizontal wire, he watches its transit over the two first vertical threads; then, suddenly intercepting the light, makes a diagram of all the stars in the field down to the 12th magnitude; and this he performs with sufficient expedition to enable him, on restoring the light, to observe the transit of his principal star over the fourth and fifth threads. The author has appended to the description of his method explanatory drawings, displaying 600 fields, of which the principal star in each has its right ascension and declination determined. He subjoins some remarks on the rate of clocks, as influeneing the observations on the upper, lower, and opposite passages; and proposes a plan for a systəm of symbols expressive of the relative magnitude of the stars recorded in his catalogue.

The author further states as one of the most important results of his researches the probable existence of a general and curious law of position in the stars, namely, that they are distributed in pairs; each star having a corresponding one in the opposite meridian, very nearly of the same declination and magnitude; a coincidence which he considers as extremely favourable to the execution of his project for the accurate determination of the position in the heavens of every star.

19. "On the Composition and Decomposition of Mineral Waters" By the Rev. George Cooke, LL.B. Commnnicated by J. G. Children, Esq., Sec, R.S.

20. "Inquiries concerning the Elementary Laws of Electricity," Part II. By William Snow Harris, Esq., F.R.S.

21. "A New Theory of the Constitution and Mode of Propagation

of Waves on the Surface of Fluids." By H. J. Dyar, Esq. Communicated by Edward Turner, M.D., F.R.S.

The Society adjourned over the long vacation, to meet again on the 17th of November.

GEOLOGICAL SOCIETY.

Nov. 2, 1836.—A paper was read, entitled "A general sketch of the Geology of the western part of Asia Minor," by Hugh Edwin Strickland, Esq., F.G.S.

This memoir embodies the observations made by the author during a winter's residence at Smyrna, a tour into the valleys of the Meander and Cayster, and a journey from Constantinople up the river Rhyndacus into Phrygia, and thence down the valley of the Hermus to Smyrna. In the latter excursion he was accompanied by Mr. Hamilton, one of the Secretaries of this Society, to whom he acknowledges himself indebted for a zealous cooperation.

The country, thus visited, is thickly beset with mountains, some of which are arranged in five parallel chains having, on a great scale, nearly an east and west bearing, but the remainder are variously grouped and without any particular direction. Four of these parallel chains bound the valleys of the Hermus, the Cayster, and the Meander; and the fifth, commencing with Mount Ida, extends eastward to the Mysian Olympus, and probably is continued in the Bithynian Olympus. With respect to the theories which have been advanced relative to the direction of a range being a mark of its comparative antiquity, the author says that the whole of the mountains of this part of Asia Minor, whether parallel or not, appear to have been elevated at nearly the same geological epoch.

The formations of which the country is composed, Mr. Strickland arranges in the following chronological order, but he states that further researches may require it to be modified: 1. Granitic rocks; 2. Schistose and metamorphic rocks; 3. Greenstone; 4. Silurian rocks; 5. Hippurite limestone; 6. Tertiary lacustrine limestone; 7. Tertiary marine formations; 8. Trachytic and trap rocks; 9. Modern volcanic rocks; and 10. Modern aqueous deposits.

1. *Granitic rocks* were not observed *in situ*, but on the authority of M. Fontanier, M. Texier, and other travellers, the loftiest part of Ida, the Mysian Olympus, the range of the Bithynian Olympus, Mount Dindymus, the top of Mount Tmolus, and Mount Latmus are granitic.

2. *Schistose and metamorphic rocks.*—This class of formations, constituting nearly all the mountain chains, consists principally of mica-schist associated irregularly with beds of marble and quartz rock, and is supposed by the author to be altered clays, earthy limestones, and sandstones. The marble is very generally distributed, but the chief points mentioned in the paper are the quarries in the island of Proconnesus, from which the name of Marmora was given to the neighbouring sea, Broussa, Ephesus, the north and west sides of Mount Olympus, and the valley of the Cayster. The colour is white, gray, or striped, and thin seams of mica often traverse the blocks, giving them a tendency to split into slabs. The quartz rock is in-

terstratified with the slate, into which it frequently passes. The strike of the beds commonly coincides with that of the mountain range, but the amount and direction of the dip is said to vary greatly.

3. *Greenstone.*—It is with some hesitation that the author gives a distinct place to this rock, as he conceives that it may be of the age of the trachytes. He observed it between Kesterlék and Adrianós, associated, though not clearly, with the mica slate; and near the village of Eshen he noticed a vein of greenstone traversing a tertiary rock, and therefore believes that the extensive greenstone formation around that village may be tertiary.

4. *Silurian rocks.*—A formation of schist and limestone containing many fossils resembling in general character those of the Silurian rocks, was observed on both shores of the Bosphorus north of Constantinople. Mr. Strickland stated that the formation would be described in a separate memoir.

5. *Hippurite limestone and schist.*—This term is employed by the author to designate the vast series of limestones, which covers a great area in the South of Europe, and represents in Asia Minor the whole of the secondary formations. On the south side of Lake Apollonia the deposit consists of compact, yellowish, lithographic stone, identical with that of Greece; at Mount Tartalí, on the east of Smyrna, of compact, gray limestone, abounding with large Hippurites, and of greenish schistose sandstone like some of the Italian macignos; on the eastern part of Mount Sipylus, above Magnesia, as well as in the peninsula of Carabornou, and in the island of Scio, it also consists of gray compact limestone; and at Mount Corax, west of Smyrna, of schistose marls and sandstone apparently devoid of fossils.

In addition to these localities, Mr. Strickland says, that on the south side of the Hermus, between Ghiédiz and Húshak, he and Mr. Hamilton observed a series of beds consisting chiefly of micaceous sandstone finely laminated, and containing occasionally beds of rolled pebbles and soft white limestone; and though the deposit is unlike any other in Asia Minor, yet he is inclined to class it with the Hippurite limestone.

6. *Tertiary lacustrine limestone.*—In the part of Asia Minor described in this paper, every large valley, with the exception of the Cayster, contains remains of extensive lacustrine deposits, forming occasionally rounded hills several hundred feet high; but they are totally wanting in the narrow ravines. They consist generally of horizontal beds of calcareous marl, sandstone, and white limestone, which is often identical in composition with English chalk, inclosing layers and nodules of flint; but sometimes approaching in character to the Italian scaglia. Near the skirts of the deposits the marls and limestones gradually become sandy and gravelly, resembling in some instances a shingle beach. The fossils noticed in these beds belong to shells of the genera Unio, Cyclas, Lymnæa, Planorbis, Paludina, and Helix, and to leaves of dicotyledonous plants.

As far as the author's observations extended, these testaceous remains resemble more the existing freshwater shells of the North of Europe than those now inhabiting Asia Minor. Thus the genus Cyclas, common in the North of Europe, was not noticed by him in Asia

Minor except in a fossil state; and the genus Melanopsis, abundant in every stream in the country, was not found in the tertiary strata.

The author then gives a detailed account of each lacustrine deposit, designating it by the name of the valley in which it occurs, or the principal town in its vicinity. He terms them the basins of Moudania, Doondár, Harmanjík, Taushanlí, Gozulják, Azani, Ghiédiz, Husháh, Sardis, Smyrna, and the lower vale of the Meander.

7. *Tertiary marine formations.*—Accumulations assigned to this class, are stated to occur on the coast of the Troad, both banks of the Dardanelles, and in the southern part of Tenedos, but they were not examined by the author.

8. *Trachytic and trap rocks.*—Patches of these rocks are scattered abundantly over Asia Minor, and are commonly associated with the lacustrine deposits, which in some cases appear to be older, in others younger, than the igneous rocks. The following are the points at which they were observed by Mr. Strickland and Mr. Hamilton in the journey from Constantinople to Smyrna: Both sides of the Bosphorus, a few miles north of Constantinople; the promontory of Bozbornou, north of the gulf of Moudania; Hammamlí near Kirmasteu on the Rhyndacus; between Debrént and Taushanlí, where volcanic matter is intermixed with a lacustrine sandstone; the vicinity of Ghiédiz, where a basaltic mass has sent forth a coulée of columnar amygdaloid, which is 10 feet thick, and rests upon beds of sand and gravel inclosing pebbles of trachyte; Gunáy; the hills west of Kobek; about 8 miles from Adala, on the road to Koola; the western side of Mount Sipylus; and the hills immediately above Smyrna.

9. *Modern volcanic rocks.*—These were observed by the author only in the Catacecaumene, and are termed by him modern, with reference to geological epochs and not to historical events. He refers them to two ages, marked by the different degree of preservation of the cones of scoria and by the appearance of the streams of lava which have flowed from them. The older cones, 30 in number, are low and flat; their craters have either disappeared or are marked by a small depression, and they are covered almost invariably with vineyards producing the Catacecaumene wine. The streams of lava connected with them are also level on the surface and covered with turf. To the north of the Hermus the author observed many isolated hills of lacustrine limestone, capped by beds of lava or basalt, which he considers may have flowed from these older cones.

The newer volcanos, of which there are only three, must have been extinct for at least 3000 years; yet their craters are perfectly defined, and their streams of lava are black, rugged, and barren. One of these craters, visited by the author, is called Karadewit or the Black Inkstand, and is about $1\frac{1}{2}$ mile north of Koola. It is a vast mound of reddish scoriæ and ashes, has a small crater on the north side, and an immense sea of black lava containing olivine and augite has flowed from its base.

As an additional proof of the comparatively great antiquity of these modern volcanic eruptions, Mr. Strickland describes the effects of a stream of lava at Adala, a town in the north-east extremity of the plain of Sardis. The Hermus enters this plain from the Catacecau-

mene through a narrow gorge between hills of mica schist. A coulée, probably derived from the most western of the three newest cones, has flowed through this gorge and expanded over the plain at Adala. The Hermus, thus impeded in its course, appears to have flown over the lava, the surface of which is smooth and bears a stratum of pebbles. In course of time the river has worn a channel between the mica slate and the lava to the depth of 80 feet, completely cutting through the coulée; yet so compact is the lava which has escaped the action of the stream of water, that it exhibits not the slightest tendency to decomposition.

The author then points out the strong resemblance between the structure of the Catacecaumene and the volcanic district of Central France. In each country are extensive lacustrine formations, cones of scoriæ of different ages, coulées, sometimes forming plateaux on the summits of isolated hills, at others continuous streams, and thick beds of lava worn through by the action of running water.

10. *Modern aqueous deposits.*—Under this head a description is given of the travertine deposited by hot springs between the foot of Mount Olympus and Broussa, forming an accumulation 2 miles in length, and at the latter locality half a mile in breadth and 100 feet high. The water has a temperature of 184° of Fahrenheit, but at present there are no springs except those at the foot of Mount Olympus.

A description was next given of the changes which have been produced by sedimentary matter deposited near the mouths of the rivers. Thus the island of Lade, once the scene of a sea-fight between the Persians and the Ionians, is now a hill in the midst of a plain; the Latmic Gulf is changed into an inland lake; the once flourishing town of Miletus, losing its harbour, is become a heap of ruins; the port of Ephesus is converted into a stagnant pool; and the delta of the Hermus threatens in a few centuries to destroy the harbour of the prosperous city of Smyrna.

The memoir concluded with the description of a recent lacustrine deposit in the valley of the Rhyndacus above Kirmasteu, which appears to have been for the greater part removed by the action of that river, only detached platforms, 50 or 60 feet high, being left on the sides of the valley.

LINNÆAN SOCIETY.

Nov. 1, 1836.—Specimens of the *Spartina glabra*, a grass new to the British Flora, were presented by Dr. Bromfield, by whom it was discovered during the past summer on the muddy banks of the river at Southampton, growing in great abundance intermixed with the *S. stricta*. The species had been previously found only in North America. It is now in such plenty in the Southampton station that, if really introduced by ballast or other means, it must have been long since naturalized.

A paper was read, entitled, Observations on the *Esula major Germanica* of Lobel. By Edward Forster, Esq., Treas. and V.P.L.S., F.R.S., &c.

Lobel in his *Stirpium Historia*, published in 1576, and Johnson

in his *Mercurius Botanicus*, published in 1634, record a species of *Euphorbia*, as growing near Bath, which Mr. Forster has satisfactorily proved to be the same with the plant published in the "Supplement to English Botany" under the name of *pilosa*. It is also recorded by Merrett in his *Pinax*, and in the *Indiculus Plantarum Dubiarum*, inserted by Dillenius at the end of his edition of Ray's *Synopsis*. The station as given by those authors answers pretty nearly to the locality in which the plant is now found. Mr. Forster regards the *E palustris* and *pilosa* as forming but one species; the circumstance of the leaves being either glabrous or hairy he considers as alone insufficient to constitute a specific difference.

A paper by Mr. Robert H. Schomburgk was also read, On the tree from which the Indians of the Oroonoko prepare the famous poison called Wooraly or Ourary. The tree proves to be an undescribed species of *Strychnos*, and it is worthy of remark that Dr. von Martius found that the Indians of the Amazon prepare a similar poison from a nearly related species of the same genus. The mode of preparing the poison appears to be confined to the Macoosies of Pirarira, and the Warpeshanas of the Conocon mountains situated near the equator, where the plant grows wild. The following are the name and character of the species:

STRYCHNOS TOXIFERA, *Schomb.*

S. foliis ovato-lanceolatis acuminatis 3—5-nerviis utrinque ramulisque ferrugineo-tomentosis, baccâ polyspermâ.

Nov. 15.—A singular specimen of an Orchideous plant was presented from Mr. Schomburgk, bearing on the same spike flowers of *Myanthus barbatus* and *Monachanthus viridis* of Lindley, which appear to be nothing more than conditions of the same species, arising from sexual differences in the flowers. The spike has five flowers of *Monachanthus*, and two of *Myanthus barbatus*. The former remain in their normal position, but the latter are resupinate. The same plant produced a second scape with all the flowers of *Myanthus barbatus*. In a letter which accompanied the interesting specimen above mentioned, Mr. Schomburgk mentions a second instance of the same kind which came under his notice, and that a vigorous plant which bore at one time flowers of *Monachanthus viridis*, had, two months previously to his writing his letter, produced a scape with flowers of *Catasetum tridentatum*, which he regards as a third condition of the same species. He states that he has never observed *Catasetum tridentatum* bear seed, but the flowers of *Monachanthus viridis* abundantly. This last would seem to be the hermaphrodite plant, *Myanthus barbatus* the female, and *Catasetum tridentatum* the male. These facts throw an entirely new light on the structure and œconomy of this remarkable family of plants.

A letter from Mr. Nicholson, addressed to the Secretary, was read, giving an account of a young Hawfinch (*Coccothraustes europæa*) just fledged, but unable to fly, having been picked up off the ground in a wood at Lullingstone in Kent, in the month of June last. Its cries

on being taken brought the two parent birds to its assistance, and they continued to fly round in circles, uttering piercing cries, for a considerable time. The bird is usually a spring visitant, and but very rarely continues throughout the summer, and breeds in this country. Neither Latham nor Montagu mentions an instance of its continuing and breeding with us.

Read also a description of the *Pithecia leucocephala* of Geoffroy St. Hilaire, the Saki and Yarke of Buffon. By Robert H. Schomburgk.

This monkey belongs to the *Platyrrhini*, a tribe which comprises all the American forms, with the exception of the genera *Jacchus* and *Midas* (the Marmzeit monkeys). The adult male is of a shining black, except the face, and the female is of a brown colour. It is a native of the interior of British Guiana, about a day's journey from the banks of the Rupununy, where they were observed by Mr. Schomburgk in considerable numbers.

Dec. 6.—Flowering specimens of the sea-side grape (*Coccoloba pubescens*) were exhibited from the Botanic Garden at Cambridge.

A. B. Lambert, Esq., V.P., exhibited specimens of two sorts of the Peruvian grain called Quinoa, from his garden at Boyton House, Wilts, one of which, the dark-coloured kind, he regards as a distinct species, for which he proposed the name of *Chenopodium altissimum*. The stems exhibited to the meeting were upwards of 12 feet in height.

Mr. Ward, F.L.S., exhibited specimens of two remarkable parasitic plants, one the *Aphyteia Hydnora*, from the Cape of Good Hope, and related to the gigantic *Rafflesia* of the Indian Islands; the other *Cynomorium coccineum* from the vicinity of Mount Sinai, where it is eaten by the natives. This last is also found in Malta, Sicily, and Barbary, where, however, the plant is extremely local.

Read, a notice by Mr. Lambert, on the culture of the Quinoa in Upper Peru, where, on the high plains, at an elevation of 13,000 feet above the level of the sea, scarcely any other grain is cultivated; but since the introduction of corn from Europe, the cultivation of the Quinoa has greatly diminished in Lower Peru and in Chili.

Read also, Descriptions of two species of the natural order Coniferæ. By Professor Don, Libr. L.S.

One of these is the *Pinus brutia*, a native of Brutium or Calabria, and nearly related to the maritime pine of Greece; the other is the *Araucaria Cunninghamii*, from the east coast of New Holland, observed by Banks and Solander in the first voyage of Cook, and since by Brown in the voyage of Flinders, and by Mr. Cunningham in that of Capt. King, and in the land expedition of Oxley to the river Brisbane. We subjoin the characters of these two species.

PINUS BRUTIA. *Ten.*

P. foliis geminis prælongis tenuissimis undulatis, strobilis sessilibus conglomeratis ovatis lævibus: squamis apice truncatis planiusculis umbilicatis.

Distinguished from *P. maritima* and *halepensis* by its very long wavy leaves, and by its shorter, sessile, clustered cones, with the scales depressed and slightly concave at their apex.

ARAUCARIA CUNNINGHAMII. *Art.*

A. decandra; foliis arboris junioris verticalitèr compressis spinuloso-mucronatis rectis; adultioris lanceolatis acutis imbricatis, strobilis ovatis: squamis apice acuminatis recurvatis margine membranaceo-alatis replicatis.

On the east coast of New Holland the geographical range of this species extends from the 14th to the 30th degree of latitude. In the young state no two plants can be more distinct than this and *A. excelsa*, but in the adult state they approach so near that it is difficult to draw the line of distinction between them.

XVII. *Intelligence and Miscellaneous Articles.*

ON THE CONSTRUCTION OF OBLIQUE BRIDGES.

The following letter on this subject is extracted from the *Newcastle Journal* of November 19th, 1836.

To Peter Nicholson, Esq.

SIR,—IN reference to a lecture stated in the public prints to have been delivered by Charles Fox, at the Royal Institution, a few months ago, on what he termed his "*new mode of constructing the Oblique Arch,*" and stating that "*formerly* the stones were cut by no general rule, but merely fitted to their particular place;" and further, in regard to what has been published by his desire in the "Philosophical Magazine" of April last, and, by "his permission", in "Loudon's Architectural Magazine" of June last,—I think it an act of justice to you, and I believe that every person who is acquainted with the valuable instructions which you have frequently, during a long course of years, produced for the advancement of science, will concur in the propriety of setting the public right as to whom the merit is due, for a proper and *certain* rule for the construction of the oblique arch. I therefore deem it right to state, that in your book on "Masonry and Stone Cutting," published in 1828, there is an elaborate illustration for the working of the *spiral* or *twist* upon the stones; and the explanation is so clear that Mr. James Hogg, operative mason, residing in Brandling Place, Newcastle, has *certified* that, in 1834, he built an oblique arch entirely from the instructions which are given in your book; and so certain did I feel of the practicability of your rule, that I have adopted it upon the river Coquet, at Felton, the chord of the arch being 33 feet, and the angle of obliquity 19°, and in which case the *stones were cut, or dressed previously to the erection of the centre.* Having received your approval of the arch as being in accordance with your design, I think there can be no doubt that your claim to the rule for the proper formation of the stones is prior to that of Mr. Fox; and I have yet to learn that any rule exists by which the oblique arch can be so truly built as the one which you have published. I am not aware, although I have endeavoured to learn,—and shall be glad, for the sake of the profession of which I have the honour to be a member, to discover the contrary,—that any of the oblique arches which have been erected upon any of the public or private works in the North of England, except those above mentioned, are constructed upon any *general principle*; and it is very remarkable, however much it is to be regretted, that up to the present time there

does not appear to have been any proper drawing prepared, nor the necessary practical instructions given for the *certain* construction of the oblique bridges, which have been built upon a very extensive public work since 1832; but that the contractors were suffered to exercise their own judgement in the erection of them; and in one case (decidedly the most important one of the whole,) the entire management was, it appears, left to one of the operative masons in the employ of the *contractor* for the building of the bridge. I have been particular in relating these circumstances, in order to show how little attention has been paid to the proper construction of oblique bridges hitherto, *which require the greatest care,* notwithstanding your valuable work on the subject, as well also to suggest the probability *that Mr. Fox was aware of the want of a general and proper rule in the cases alluded to*, and hence his *mistaken notion* that "formerly," that is, previously to his lecture a few months ago, "the stones were cut by no general rule, but merely fitted to their particular place."

I sincerely hope that the inquiry which has been set on foot, with a view to prove that a sure rule for the spiral formation of the stones has existed for several years, will lead to some advantage, and I am informed that already, that is, since the commencement of the inquiry, the executive engineer of a very extensive public railroad has very prudently *applied to a competent person for a definite development of your principle,* so that in *future* he may possess a safe guide; thus verifying the adage, "better late than never."

It is too clear that at the present time there is a sort of mania for oblique bridges. That in many cases, *though not so frequently as they occur,* they are indispensable upon railroad lines, but for turnpike roads, where the rate of travelling rarely reaches 11 miles an hour, and considering the absence of lateral friction, now that our roads are no longer "gridironed," and the little risk, comparatively, of departing from the proper course, so great an inconvenience in railroads, though *strikingly disregarded in various situations,* I am firmly of opinion, after an attentive observation of the *practical working of the best rule,* that it is very injudicious to adopt them, except in cases of absolute necessity. The fact too, that *your* opinion fully coincides with the one represented in the *Encyclopædia Britannica,* article "Stone Masonry," viz. "*that oblique bridges should be avoided whenever it is possible,*" is, I submit, a very strong proof of their inferiority. I am, Sir, very respectfully,

Your obedient servant,

Henry Welch,

Surveyor of the County Bridges of Northumberland.

Elswick Villa, Newcastle, Nov. 17, 1836.

Mr. Fox's paper alluded to above appeared in Lond. and Edinb. Phil. Mag., vol. viii. p. 299.

OBSERVATIONS ON THE AURORA BOREALIS.

1. *Aurora visible at Dublin on Thursday, 29th September,* 1836.

The 29th of September last was a cloudy and showery day, but the

sky partially cleared before sunset, and though large patches of rain cloud hung about, the sun set with a clear *red* horizon.

The evening was cold; stars visible; the wind gentle, N. or N. by W.

At $6^h\ 40^m$ a broad diffused light appeared in the northern hemisphere; it gradually became more intense, and at 7^h had lowered its upper and raised its under edges, and assumed the form of a well-defined arch or segment of a circle. The apparent altitude of the upper edge was about 30°, and the breadth about 8° or 10°. The eastern wing bore about N.N.E., and the western about S.E.E., both resting on the horizon.

The light of the arch was white, and the stars were visible through it; there were no clouds in its neighbourhood, nor any waves or alterations in the intensity of its light, nor any black band.

At a few minutes past 7 a lofty vertical column of yellowish white light shot upwards from the horizon, at its eastern side, to the zenith, where it was lost. Its edges were perfectly straight and exactly defined, particularly the western. It soon became faint from below, and the upper part resolved itself into a cloudy mass of deep red light, remarkable for depth and brilliancy of colour, being equal to that of the richest red stained glass. In a few seconds a similar column rose about 60° in altitude on the western side, and almost at once became a broad cloud of rich rosy light, so bright as to appear like the reflection of a near and great conflagration. Through this, white vertical streamers several times shot. Towards the zenith *the stars appeared red through it.*

The arch now began to depress itself, and the light gradually faded, first of the two cloudy columns, and of these the eastern first, and afterwards the arch itself. By $8^h\ 10^m$ it had all disappeared and the night become overcast. By 11^h it had become so clouded that no stars were visible. There was no other meteor seen.

The altitudes and bearings given are to be considered merely as approximations, as from the shortness of duration of the aurora and other circumstances I was enabled to make no instrumental observations, and my reason for communicating results having such little pretensions to exactness is to draw attention to the recurrence of red auroras at this early season, one having been observed at Ryde, as noticed in the last Number of the Phil. Mag., and another, but of a less important character, having been visible to night (October 5), at $7^h\ 35^m$. It would appear not unimportant to observe the colour of the sky at sunset, at all times when possible, in connexion with the colour of auroras, as this might afford some indication how far the colour was due to atmospheric interference, and possibly give some indirect clue in solving the question of the altitude of the region of auroral phænomena. ROBERT MALLET.

Dublin, October, 5, 1836.

2. *Aurora observed at Dublin, October* 11, 1836.

This aurora was different to three others seen this season and the many observed last winter. Captain Back, in his Journal, describes and figures one which very much resembled that seen last evening,

which was like to the tail of a vast comet. I cannot say when it began, but I noticed it very strong and distinct on going out of doors exactly at 8 o'clock from Gardiner's Row; it then appeared to be altogether S. of the zenith. It appeared to begin about 15° S.E. of the Pleiades, and passing them at about 10° of distance, and thence extending about 90° N.W. From our zenith its diffused western end may have been from 30° to 40°. At this place there was a cirro-stratus cloud, which appeared towards the close of the aurora, about 12 minutes after 8, also to be illuminated. This aurora was apparently narrower at its eastern termination than its west; it was also arched; it had a sensible motion of about 1° per minute S. At 5 minutes past 8 its edges presented that peculiar blackness which has been noticed by different persons in the Canadian aurora, supposed to arise from the formation of sensible vapour. This aurora was of that kind which Capt. Back and Mr. Farquharson, and others, have noticed as arising from the tops of mountains in masses of cloud. If some of your correspondents in the south of the city would communicate their observations, we might, perhaps, be able to trace the locality of its eastern terminus, and determine its height. Recent observations lead us to trace the course of auroræ to previous and distant thunder storms. There were appearances of a thunder storm towards the N.W. yesterday in the afternoon, and at 20 minutes to 5 I distinctly perceived the reflection of a flash of lightning when walking in Eccles street.—*Oct.* 12. E. C. (*Saunders's News-Letter, Thursday, Oct.* 13, 1836.

3. *Aurora of October* 11, *as observed at Leominster.*

To the Editors of the Philosophical Magazine and Journal.

I take leave to send you the following notice of a splendid and (as it appeared to me) a highly interesting electrical phænomenon, seen here last Tuesday night, the 11th of October, about a quarter before nine; thinking, as it was visible only for a few minutes, you might, perhaps, consider it worth inserting in your Journal. The sky was clear at the time, except on the eastern and western horizon, where it was obscured by clouds to the height of about 40°. A broad stream of light, of a pale yellow colour, stretched across the heavens, passing through the zenith, connecting, like an immense bridge, the banks of clouds in the east and west. It first appeared as a band of light streaming in a direct line from the masses of clouds in the west; it quickly assumed a waving appearance, forming a variety of graceful curves through nearly the whole extent; this lasted about ten minutes, when it became fainter at the eastern end, and gradually faded away towards the west. In a few minutes more it disappeared altogether.—Wm. Gilks.

Leominster, Oct. 13, 1836.

PROCESS OF MAKING CRYSTALLIZED SUGAR FROM TODDY, OR THE JUICE OF THE COCOA-NUT PALM, ON THE ISLAND OF CEYLON.

The following notice on this subject was communicated by Lieut.-

Colonel Colebrook, Royal Artillery, M.R.A.S., to the Royal Asiatic Society, and appears in No. VI. of that Society's Journal.

"The toddy is collected in vessels perfectly clean, into each of which a small quantity of the *ál*, or banyan-tree, is put to retard fermentation and correct astringencies. Before the liquor begins to ferment, it is strained through a clean cloth, and boiled in a pan of brass, or other metal, until the impurities rise to the surface, when they are carefully skimmed off. When the liquor has lost its watery colour and become a little reddish, it is poured into another pan, and boiled over a strong fire, the scum being again taken off as it accumulates. The fire is then gradually diminished, until a white scum appears on the surface and increases to a froth. The liquor then becomes adhesive, and of a consistency to be removed from the fire, which is ascertained by allowing a little of it to cool, and by drawing it into a thread between the finger and thumb. If the thread does not break when drawn to about an inch in length, the syrup is removed from the fire, poured into another vessel, and left to cool till it is little more than lukewarm. A little crystallized *jagrí*, or coarse sugar-candy, is then mixed with it, and the whole is poured into a fresh vessel, having an aperture and stopple in the bottom, so accommodated as to allow the uncrystallized part to ooze out. Crystallization is completed in about a week, when the stopple is removed to allow the remaining fluid to escape, and at the end of another week the crystallized sugar is taken and placed near a fire in a *góni*, or sack. The expense of manufacture is about one penny and one-eighth per pound, exclusive of the cost of vessels."

PROFESSOR RENWICK ON THE HEIGHT OF THE ROCKY MOUNTAINS OF NORTH AMERICA.

We extract the following observations respecting the altitude of these mountains, hitherto so much underrated, from the Appendix to Mr. Washington Irving's "Astoria, or Enterprize beyond the Rocky Mountains," just published, hoping to draw further attention to the subject both in Europe and in America.

"Various estimates have been made of the height of the Rocky Mountains, but it is doubtful whether any have as yet done justice to their real altitude, which promises to place them only second to the highest mountains of the known world. Their height has been diminished to the eye by the great elevation of the plains from which they rise. They consist, according to Long, of ridges, knobs, and peaks, variously disposed. The more elevated parts are covered with perpetual snows, which contribute to give them a luminous and, at a great distance, even a brilliant appearance; whence they derived, among some of the first discoverers, the name of the Shining Mountains.

"James's Peak has generally been cited as the highest of the chain; and its elevation above the common level has been ascertained, by a trigonometrical measurement, to be about eight thousand five hundred feet. Mr. Long, however, judged, from the position of the snow near the summits of other peaks and ridges at no

great distance from it, that they were much higher. Having heard Professor Renwick, of New York, express an opinion of the altitude of these mountains far beyond what had usually been ascribed to them, we applied to him for the authority on which he grounded his observation, and here subjoin his reply :

"'Columbia College, New York, Feb. 23, 1836.

"'Dear Sir,—In compliance with your request, I have to communicate some facts in relation to the heights of the Rocky Mountains, and the sources whence I obtained the information.

"'In conversation with Simon MacGillivray, Esq., a partner of the North-west Company, he stated to me his impression, that the mountains in the vicinity of the route pursued by the traders of that company were nearly as high as the Himalayas. He had himself crossed by this route, seen the snowy summits of the peaks, and experienced a degree of cold which required a spirit thermometer to indicate it. His authority for the estimate of the heights was a gentleman who had been employed for several years as surveyor of that company. This conversation occurred about sixteen years since.

"'A year or two afterwards, I had the pleasure of dining at Major Delafield's, with Mr. Thompson, the gentleman referred to by Mr. MacGillivray. I inquired of him in relation to the circumstances mentioned by Mr. MacGillivray, and he stated that, by the joint means of the barometer and trigonometric measurement, he had ascertained the height of one of the peaks to be about twenty-five thousand feet, and there were others of nearly the same height in the vicinity. I am, dear Sir, yours truly,

"'*To W. Irving, Esq.* James Renwick.'"

METEOROLOGICAL OBSERVATIONS FOR NOVEMBER 1836.

Chiswick.—Nov. 1, Frosty: cold and wet: cloudy. 2. Hazy: drizzly. 3. Cloudy: fine: rain at night. 4. Overcast: rain. 5. Cloudy. 6, 7. Frosty. 8. Sharp frost: clear: frosty with fog at night. 9. Overcast: rain. 10. Cloudy: fine. 11. Heavy rain: cloudy and fine. 12. Foggy. 13. Rain: fine: rain at night. 14. Clear and fine. 15. Clear and frosty: cloudy. 16. Fine. 17. Overcast: very fine: stormy with rain at night. 18. Fine. 19. Slight frost: rain: stormy showers. 20. Clear. 21. Foggy: drizzly. 22. Foggy: rain. 23. Dense clouds: boisterous: clear at night. 24. Clear and cold. 25. Frosty: hazy: rain. 26. Foggy: rain. 27. Cloudy. 28, 29. Boisterous with rain. 30. Fine: rain.

[The barometer on the 8th, at 8 A.M., was 30·083, and had risen at an average of ·028 per hour during the night; consequently would be about 30 inches at the time when that along with Mr. Green, in his balloon ascent, was at 20 inches, or —$\frac{1}{3}$ of the whole atmospheric pressure near the level of the sea.—R. Thompson.]

Boston.—Nov. 1—3. Cloudy. 4. Fine. 5. Cloudy. 6—8. Fine. 9. Cloudy: rain early A.M. 10. Fine: rain early A.M.: rain P.M. 11. Cloudy. 12. Foggy. 13. Rain: rain P.M. 14—16. Fine: rain P.M. 17. Cloudy: rain P.M. 18. Fine: rain P.M. 19. Rain: rain P.M. 20—22. Fine: rain P.M. 23. Stormy: rain early A.M. 24. Fine. 25. Cloudy. 26. Cloudy: rain early A.M.: rain P.M. 27, 28. Cloudy: rain early A.M.: rain P.M. 29. Rain: rain early A.M.: rain P.M. 30. Fine: hurricane with rain A.M.

Nov. 28th, about one o'clock P.M., during a heavy thunder storm, Boston church steeple was struck by lightning, which did considerable damage to one of the pinnacles, and one of the vanes fell to the ground.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary; by Mr. THOMPSON at the Gardens of the Horticultural Society at Chiswick, near London; and by Mr. VEALL at Boston.

Days of Month. 1836. Nov.	Barometer. London: Roy. Soc. 9 A.M.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ A.M.	Thermometer. London: Roy. Soc. Fahr. 9 A.M.	Thermometer. London: Roy. Soc. Self-registering. Min.	Thermometer. London: Roy. Soc. Self-registering. Max.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8½ A.M.	Wind. London: Roy. Soc. 9 A.M.	Wind. Chisw. 1 P.M.	Wind. Bost.	Rain. London: Roy. Soc. 9 A.M.	Rain. Chisw.	Rain. Boston.	Dew-point. Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
☾ T. 1.	30·099	30·157	30·015	29·68	35·2	31·7	37·8	53	38	35	SSW.	SW.	calm	...	·07	...	32
W. 2.	29·942	29·979	29·939	29·42	47·7	34·8	53·2	55	35	48·5	SSW.	SW.	calm	·044	·02	...	39
Th. 3.	29·840	29·892	29·665	29·30	44·8	42·3	51·7	54	39	41	SW.	SW.	calm	...	·22	...	39
F. 4	29·570	29·607	29·369	29·05	44·5	40·9	46·4	47	41	41	SW.	SW.	calm	·080	·14	...	40
S. 5.	29·140	29·456	29·157	28·73	45·4	42·8	46·2	49	27	39	SW.	NW.	calm	·122	...	...	40
⊙ 6.	29·459	29·498	29·417	29·05	36·4	34·0	43·8	47	28	36	WSW.	SW.	calm	...	...	...	36
M. 7.	29·631	29·853	29·658	29·17	35·8	32·3	43·2	45	23	36	WSW.	SW.	calm	·055	...	...	35
T. 8.	29·978	30·107	30·068	29·60	33·6	31·3	45·8	52	26	32·5	SW.	SW.	calm	...	·05	...	33
● W. 9.	30·010	30·051	29·740	29·57	45·8	33·5	50·3	55	45	42	SSE.	SW.	calm	·036	·28	·17	36
Th. 10.	29·614	29·660	29·634	29·10	50·4	45·8	53·8	56	33	49	S.	SW.	calm	·211	·30	·29	43
F. 11.	29·390	29·665	29·414	29·03	50·5	42·6	50·6	51	35	47	E.	S.	calm	·136	·10	·07	44
S. 12.	29·821	29·879	29·852	29·35	41·4	40·2	52·0	53	36	43	SW.	SW.	calm	·052	·14	...	41
⊙ 13.	29·687	29·687	29·543	29·10	52·2	40·7	54·9	56	38	54	SE. var.	S.	SW.	·038	·17	...	45
M. 14.	29·656	29·712	29·659	29·11	42·7	41·0	46·0	47	32	41	SW.	W.	calm	·216	...	·13	44
☽ T. 15.	30·011	30·163	30·018	29·47	38·4	35·9	46·7	48	38	39	SW.	SW.	calm	...	...	...	41
W. 16.	29·964	29·989	29·636	29·47	46·7	37·9	50·2	51	44	42·5	SW.	SW.	calm	...	·03	·03	42
Th. 17.	29·475	29·483	29·142	28·90	46·7	45·2	47·5	50	38	46	SW.	SW.	W.	...	·08	...	43
F. 18.	29·103	29·242	29·114	28·72	40·8	39·8	44·0	48	31	38·5	WSW.	W.	calm	·069	...	·18	42
S. 19.	29·253	29·310	29·298	28·77	37·4	34·4	38·7	46	36	36	SSW.	W.	calm	...	·05	·06	38
⊙ 20.	29·784	30·084	29·831	29·45	38·7	36·9	42·7	46	30	38	ENE.	NE.	calm	·180	...	·37	37
M. 21.	30·119	30·165	30·038	29·55	36·7	32·6	41·5	41	34	36	SW.	SW.	calm	...	·08	...	34
T. 22.	29·960	29·987	29·647	29·55	38·8	33·8	49·5	51	37	36	NW.	SE.	calm	·069	·16	·05	37
○ W. 23.	29·272	29·504	29·301	28·70	44·6	37·9	44·3	46	36	42	SW.	NW.	W.	·163	...	·34	38
T. 24.	29·479	29·624	29·516	29·06	38·5	36·6	43·6	44	25	37	SW.	W.	W.	...	...	...	36
F. 25.	29·732	29·793	29·657	29·35	35·7	33·5	39·3	41	32	33	WNW.	SW.	calm	...	·16	...	35
S. 26.	29·356	29·801	29·330	29·06	39·7	34·5	50·7	50	40	38	E.	SE.	NE.	·136	·16	·19	36
⊙ 27.	29·530	29·581	29·458	29·04	44·5	39·0	55·3	56	51	48	SW.	SW.	calm	·158	·07	·25	40
M. 28.	29·267	29·284	29·250	28·64	55·4	49·2	56·7	58	47	55	S. var.	SW.	calm	·033	·45	·11	48
T. 29.	28·954	29·525	29·016	28·40	56·8	52·2	52·6	53	41	51·5	S. var.	W.	E.	·183	·56	·49	51
W. 30.	29·516	29·693	29·495	29·05	48·4	47·5	49·3	48	32	43	S.	NW.	calm	·127	·37	·73	46
	29·620	30·165	29·016	29·14	43·1	38·7	47·6	58	23	41·4				Sum 2·108	3·60	3·46	39·7

THE

LONDON AND EDINBURGH

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[THIRD SERIES.]

FEBRUARY 1837.

XVIII. *Notice relative to the Publication of the* SCIENTIFIC MEMOIRS. *By* RICHARD TAYLOR.

I AVAIL myself of the opportunity which the Philosophical Magazine affords me, to announce the publication of the Third Part of the subsidiary work in which I have lately engaged, the SCIENTIFIC MEMOIRS, and to appeal for support and assistance to all those who may be satisfied of the utility and necessity of such an instrument for furthering the progress of scientific pursuits in this country, by giving readier access to the labours of foreign philosophers.

The views with which the work was undertaken were thus stated in the Advertisement prefixed to the First Part:

"In order to bespeak the favour of the public to this new undertaking, I shall merely state that the want of such a work has been suggested to me, during a long connexion with journals of science, by my experience of the difficulty of giving to the English reader, within the necessary limits of those works, a sufficient view of the state and progress of the sciences in other countries. Short abstracts are given, and now and then entire translations of important memoirs; but original communications, and the scientific proceedings of our own country, now occupy so great a space in our journals as to make it impossible to do justice to the researches of foreigners. Many, indeed, are the valuable memoirs in which these are recorded, which have furnished subjects for investigation and discussion among the most distinguished of our countrymen, but which in their entire form have never appeared in English, and can be referred to only in a small number of public libraries.

"Projects for supplying this deficiency have been entertained at various times both by societies and individuals: the scientific journals, also, which have successively existed have been looked to with this view. But experience seems to have proved that a regular and full supply of such matter is not consistent with their plan or limits; and a considerable increase of their bulk and cost might reduce the number of purchasers, and

lessen the support which in this country is already scarcely sufficient to keep them in existence. I have thought it better therefore to try the experiment of a separate work devoted entirely to the publication of the more important memoirs and communications, translated from foreign transactions and journals, to appear at intervals sufficiently distant to avoid the disadvantage of haste in the execution, and yet frequent enough to keep pace with the progress of discovery and the wants of the student.

"My present intention is, that the quarterly parts should form an annual volume, which may possess a permanent value, as a depository of those memoirs in which philosophers of other countries have fully developed and carefully recorded their views and their discoveries. With regard to the class or arrangement of subjects, I have thought it best not to confine myself at the outset within any fixed plan of selection; but to endeavour from time to time, with the advice and assistance of men of science in our own country, to make it subsidiary to their pursuits, and illustrative of the objects of inquiry which most engage their attention.

"I have been greatly encouraged by the opinions which have been expressed to me as to the probable usefulness of a work of this kind, as well as by the valuable suggestions and assistance with which I have been favoured; and, should the patronage of the public be sufficient to enable me to persevere, I may have the satisfaction of making my occupations conducive to the interests of science."

How far the execution of the design may have hitherto been generally satisfactory I know not. The approbation and patronage of many distinguished persons of the highest scientific eminence greatly encourage me to persevere; but, as yet, the number of purchasers has not been nearly sufficient to defray the cost of publication. I do not hesitate, therefore, to make an appeal to all who may participate in the wish for the continuance of the work, trusting that they will lend their active aid in promoting its success. With a demand sufficient to cover the expenses, I should determine to proceed, being more than ever convinced of the need of such a work, and having made various arrangements for its improvement.

The following are the Memoirs contained in the First and Second Part:

M. Melloni, On the Free Transmission of Radiant Heat through different Solid and Liquid Bodies;

New Researches relative to the Immediate Transmission of Radiant Heat through different Solid and Liquid Bodies; and,

Memoir on the Polarization of Heat. (From the *Annales de Chimie et de Physique.*)

Prof. H. W. Dove, Experiments on the Circular Polarization of Light; and,

Description of an Apparatus for exhibiting the Phænomena of the Rectilinear, Elliptic and Circular Polarization of Light. (From Poggendorff's *Annalen der Physik und Chemie.*)

M. Nobili, A Memoir on Colours in general, and particularly on a new Chromatic Scale deduced from Metallochromy for Scientific and Practical Purposes. (From the *Bibliothèque Universelle des Sciences,* &c.)

M. Poisson, On the Mathematical Theory of Heat. (From the *Annales de Chimie et de Physique.*)

M. Savart, Researches on the Elasticity of Bodies which crystallize regularly. (From the *Annales de Chimie et de Physique.*)

Prof. LÖWIG, Experiments on the Essential Oil of the *Spiræa Ulmaria,* or Meadow Sweet. (From Poggendorff's *Annalen.*)

Baron WALCKENAER, Researches relative to the Insects known to the Ancients and Moderns by which the Vine is infested, and on the Means of preventing their Ravages. (From the *Annales de la Société Entomologique de France.*)

Dr. CARUS, The Kingdoms of Nature, their Life and Affinity. (From the *Zeitschrift für Natur und Heilkunde.*)

J. A. BALARD, Researches concerning the Nature of the Bleaching Compounds of Chlorine. (From the *Annales de Chimie et de Physique.*)

E. LENZ, On the Laws of the Conducting Powers of Wires of different Lengths and Diameters for Electricity. (From the *Memoirs of the Academy of Sciences of St. Petersburg.*)

Among these it will be seen that the entire series of Melloni's important investigations are now first given to the English public. In order to render the work useful and interesting, I have been advised to give a preference to subjects which engage immediate attention in this country. Of this kind are the labours of M. Becquerel upon the artificial production of crystallized minerals by means of voltaic action. The communications of Mr. Fox and Mr. Crosse at the late meeting of the British Association at Bristol have excited much interest upon this subject, at the time and subsequently. (See pp. 228 and 537 of our preceding volume.) What had been done by Becquerel with regard to it appears, however, to have been unknown both to those gentlemen and a great part of their auditors, the only accounts of his experiments that had appeared in this country being our short notice in 1830, (Philosophical Magazine and Annals, vol. xii. p. 226,) and that in Mr. Whewell's Report on Mineralogy. In preparing for publication M. Becquerel's paper I have been favoured with the valuable assistance of Mr. Brooke and Mr. Golding Bird: and for the highly important Memoir by Mossotti I am indebted to Prof. Faraday, who kindly communicated to me a copy which he had just received from the author. These occupy a portion of the Third Part; together with the two entire Memoirs of Ehrenberg on his discoveries of Fossil *Infusoria,* which are just now engaging much of the attention of naturalists. Two additional papers by Melloni are now given, in one of which he describes his mode of separating the rays of light from those of heat. The insertion of Von Wrede's Memoir on the absorption of light was obligingly suggested to me by Professor Powell.

List of the Memoirs contained in Part III., just published:

M. CLAPEYRON, On the Motive Power of Heat. (From the *Journal de l'Ecole Royale Polytechnique.*)

Dr. BURMEISTER, On the Sound produced by Insects in Flying. (From Poggendorff's *Annalen.*)

M. MELLONI, On the Reflection of Radiant Heat; and,

On the Theory of the Identity of the Agents which produce Light and Radiant Heat. (From the *Annales de Chimie et de Physique.*)

M. Biot, On the Constitution of the higher Regions of the Earth's Atmosphere. (From the *Comptes Rendus des Séances de l'Académie des Sciences.*)

Prof. Ehrenberg, On Fossil Infusoria.—The two Memoirs entire, with an Engraving of the Figures. (From Poggendorff's *Annalen.*)

M. Becquerel, On the artificial production of Crystallized Minerals by Voltaic action.

Prof. H. Rose, On a New Combination of Anhydrous Sulphuric and Sulphurous Acids. (From Poggendorff's *Annalen.*)

M. Mossotti, On the Forces which regulate the Internal Constitution of Bodies. (A Memoir published separately; containing the views respecting the identity of origin of gravitation, the attraction of aggregation, and electrical attraction, involving the discovery of the physical cause of gravitation, which were explained, from this Memoir, by Mr. Faraday at the first evening meeting of the Royal Institution for the present season, January 20, 1837.)

M. Pelouze, On certain Combinations of a new Acid, formed of Azote, Sulphur, and Oxygen. (From the *Annales de Chimie et de Physique.*)

Baron von Wrede, On the Absorption of Light, according to the Theory of Undulations. (From Poggendorff's *Annalen.*)

My connexions and my profession may afford me, perhaps, advantages for carrying on the work with a less amount of support than would be requisite if it were in other hands. I cannot, however, be expected to incur a considerable loss; and I look with confidence to those who approve my attempt to give me their active aid. If they wish the work to be continued, they will, I trust, exert themselves to increase the number of purchasers, which at present is far below what is absolutely necessary.

I have only to add, that the attention requisite for conducting this work I shall be happy to give, if its circulation can be so far extended as barely to pay the cost; and, though necessarily it can hardly be of a popular character, this is not a greater degree of success than may be hoped for, in this country and its dependencies.

Richard Taylor.

XIX. *Experimental Researches on the Nature and Properties of Albumen, &c. By* Golding Bird, *Esq., F.L.S., F.G.S., Lecturer on Experimental Philosophy at Guy's Hospital.*

[Continued from vol. ix. p. 115, and concluded.]

In the former part of this communication I pointed out the solubility of albumen after its coagulation by alcohol, &c., in water saturated with carbonic acid, as well as the curious fact of that acid decomposing the alkaline albuminates, causing the separation and consequent precipitation of the albumen in an insoluble form, capable of being redissolved by an excess of the acid. I also mentioned some circumstances connected with the action of coagulated albumen on the carbonates of

soda, and I now propose to continue the account of my investigations in this interesting department of organic chemistry.

9. From the result of the experiments already detailed (7.), it appeared probable that albumen after its coagulation by alcohol was capable of partly decomposing the carbonate and bicarbonate of soda. An interesting inquiry instantly suggested itself as to whether the fluid albumen possessed a similar property. To ascertain how far this was actually the case, I mixed fresh serum of human blood, freed from fat (1.), with an equal bulk of a cold solution of carbonate of soda* of spec. grav. 1·030. On the addition of the first portion of the alkaline carbonate, the serum became slightly turbid, which condition, however, ceased on agitation; the mixture was then placed in a flask furnished with a tube bent twice at right angles and dipping into lime-water; heat was then applied to the flask and continued until the mixture nearly attained the boiling-point, when it suddenly swelled up so as nearly to reach the mouth of the vessel, a torrent of bubbles at the same instant escaping through the lime-water, which became quite milky: the lime-water was then removed, and a vessel full of diluted tincture of litmus substituted; the gas continuing to be evolved, quickly converted the blue colour of the litmus to red. This experiment proves most satisfactorily the evolution of carbonic acid gas by the action of albumen on carbonate of soda; and as neither the solution of the alkaline carbonate nor of the albumen *separately* evolved carbonic acid, it follows as a necessary consequence that the albumen had been a sufficiently energetic electro-negative element to expel *part* of the carbonic acid from the soda and unite with the alkali thus set free, forming an albuminate of soda. The contents of the flask were quite transparent after the experiment, and exerted a strong alkaline reaction on turmeric. I may observe that, after repeating this experiment several times, I constantly found the evolution of the carbonic acid to occur *only* when the mixture had attained the boiling-point, and then to take place suddenly, and almost immediately cease.

10. Finding that albumen was able to expel *part* of the carbonic acid from the carbonate of soda, I attempted, by using a considerable excess of albumen, to decompose *entirely* a small portion of the carbonate; but in this I failed altogether, the albumen (like some other weakly electro-negative bodies) having never, under any circumstances which have yet fallen under my notice, been sufficiently energetic to expel *all* the carbonic acid from any given portion of the alkaline carbonate.

11. When the sesquicarbonate or the bicarbonate was

* This solution had been previously boiled for some minutes to decompose any bicarbonate of soda that might have accidentally been present.

substituted for the neutral carbonate of soda, in the above experiments, a much more considerable evolution of the acid gas occurred, as might indeed have been expected *à priori*.

12. I next repeated the experiment (10.), substituting for the carbonate of soda a (previously well-boiled) solution of carbonate of potash of nearly the same specific gravity, the bent tube with which the flask was furnished dipping into lime-water; but not the slightest opacity occurred, even after the ebullition had been kept up so long as to cause a partial decomposition of the albumen, evinced by the mixture assuming a deep-brown colour. From the result of this experiment I think it may be fairly deduced, that although albumen is capable of partly decomposing the carbonate of soda, yet that it is by no means sufficiently energetic to exert a similar action on the carbonate of potash.

13. The result of the last experiment tends to throw a considerable degree of obscurity over the nature of the solvent action exerted by carbonate of potash or albumen, for that it does exert such an action there can be no doubt, as it prevents the coagulation of albumen by heat; for while, in the case of the solution of albumen in carbonate of soda, we have sufficient data to prove that part of the carbonic acid is expelled, and an alkaline albuminate formed; yet we are unable to apply a similar explanation to the solution of albumen in carbonate of potash, in consequence of our want of evidence of its possessing sufficient energy to exert even a partial decomposing action on the latter salt: we have therefore only the alternative of supposing that it either forms a ternary combination with the carbonate of potash, or that it exists merely dissolved in the alkaline salt without (strictly speaking) chemical combination.

Action of Electric Currents of different Intensities on Albumen in its free or combined State.

14. Upon no subject connected with the products of organization has more discrepancy existed than upon the results of electric currents on albumen; one author stating that it is deposited from its solution at the negative; another, at the positive electrode; while others, as Raspail (*Nouv. Syst. de Chim. Organ.*), declare that it is coagulated equally at both electrodes; all, however, agreeing in the excessive delicacy of an electric current as a test for detecting its presence when all other means fail. But after investigating this subject with considerable care, using different intensities of electric action, solutions of albumen of different degrees of concentration, and (what I found to be of considerable importance, as influencing the results,) electrodes of different metals, I feel myself justified in stating, that the action of electric currents on albumen

is constant, and by no means so capricious as has been imagined.

15. Mr. Brande was, I believe, the first who applied electro-chemical action to the detection of albumen (Phil. Trans. 1809; p. 373); and from the results of his experiments it appears, that when a solution of albumen (of white of egg) was submitted to the action of a battery of 120 four-inch double plates excited by dilute nitro-muriatic acid, rapid coagulation took place at the negative, whilst only a thin film of albumen appeared at the positive electrode: the same results occured when a battery of 24 four-inch plates, "highly charged," was substituted for the larger one, provided the wires forming the electrodes were not more than half an inch distant from each other; for when they were separated to eight inches, or a smaller battery used, the albumen appeared only at the positive electrode. When albumen of pus was substituted for the white of egg, Mr. Brande found coagulation to take place at both electrodes; but the size of the battery used in this experiment is not mentioned. Albuminate of soda (procured by boiling white of egg in water), when submitted to the action of a battery of 60 four-inch plates, deposited albumen copiously at the negative electrode. From the results of his experiments Mr. Brande appears to have been induced to believe that albumen, free or combined, always coagulated with a strong electric current at the negative, and with a weak current at the positive electrode.

16. To obviate any error that might arise from the heat evolved by the passage of electric currents of considerable tension, I determined to employ small plates ($2\frac{1}{2}$ inches square) excited only by weak brine; these plates were arranged in separate jars, precisely on the principle of the *couronne des tasses.* Thirty pairs of zinc and copper plates thus arranged and excited, developed a current sufficiently energetic to decompose water and saline solutions with rapidity, and communicate a disagreeable shock extending beyond the wrists; but failed to heat perceptibly a piece of platina wire ($\frac{1}{1000}$ of an inch in diameter) sufficiently fine to be readily ignited by a single voltaic pair presenting 4 square inches of surface, excited by dilute sulphuric acid.

17. I first examined the action of an electric current on fluid albumen in as uncombined a state as it can readily be obtained: for this purpose some dilute and prepared serum of blood (1.) was placed in a glass cup, and by means of platinum wires connected with the battery of 30 pairs, excited (as in all the following experiments unless otherwise expressed) by weak brine; in a few minutes a considerable cloudy deposit occurred in the proximity of the positive electrode, *but without*

adhering to it. To determine with more accuracy at which electrode the albumen was really deposited, the experiment was repeated, with *two* cups containing a solution of albumen, connected with each other either by means of moistened cotton or asbestos, or, what appears to me to be much more convenient, by a glass tube bent twice at right angles and terminating in capillary orifices, filled with water containing just enough common salt to conduct the current with sufficient readiness to ensure the success of the experiment. In this form of the experiment copious coagulation of albumen soon appeared in the positive cup, whilst the fluid in the negative cup retained its limpidity. On examining the contents of the cups after the electric action had been continued for six hours, the fluid in the positive cup was found to be very acid, contained in diffusion much coagulated albumen, and smelt powerfully of chlorine, whilst the contents of the negative cup were limpid and alkaline, and not coagulated by heat; the addition of acetic or nitric acid, however, caused a copious precipitation of albumen. The rationale of this result is very simple: the chloride of sodium (from which it is nearly, if not quite impossible to free liquid albumen) had been decomposed; chlorine and, from the subsequent decomposition of the water, hydrochloric acid appearing in the positive, whilst the soda had been conveyed to the negative cup, where under electric action it had combined with the albumen, forming an albuminate of soda.

18. The last-described experiment was repeated, but with only 6 pairs of the same plates (16.), similarly excited by weak brine, and with platinum electrodes: gas was feebly evolved from both wires, and in about half an hour the contents of the positive cup were found to be acid, turbid (from the deposition of albumen), and *smelling strongly of chlorine*; whilst the fluid in the negative cup was alkaline, limpid, and contained albuminate of soda in solution; results precisely identical with those obtained when the larger battery was employed (17.). When copper wires were substituted for platinum in connecting the cups of albumen to the little battery, a singular and marked difference resulted; the positive wire became tarnished from the formation of an oxide, and was almost instantly covered with a film of albumen, which increased so rapidly that in a few minutes a piece of copper wire 0·02 inch thick became as thick as a crow-quill from the rapid coagulation of albumen *which adhered strongly to it*: this albumen was green, and contained a considerable quantity of oxide of copper; affording in its physical and chemical characters a marked and important difference from that obtained when platinum electrodes were substituted for copper.

19. Some of the solution of albumen was placed as before

(17.) in two cups, connected by filaments of moistened cotton, and placed, by means of platinum wires, in communication with the battery of 30 pairs (16.) in the positive cup, which I shall call A: a considerable deposit of albumen had appeared, the contents being *acid* and smelling of chlorine, whilst the contents of the negative cup, B, were limpid and *alkaline*. The connexion with the battery was now reversed, so that B became the positive, and A the negative cup. In a quarter of an hour the contents of A were found to be *alkaline* and limpid, the albumen that had been deposited having been taken up, whilst the fluid in B had in its turn become turbid (from the coagulation of albumen) *acid*, and had acquired an odour of chlorine. The rationale of this interesting experiment is too obvious to require any explanation.

20. The above experiments having been repeated many times, and always with the same results, we may safely deduce from them the inference, that whenever albumen is coagulated under electric agency, an acid (hydrochloric?) mixed with chlorine is always set free at the positive, whilst an alkali (soda?) as constantly appears at the negative electrode. Mr. Brande (*Op. sup. cit.*, p. 375 *et seq.*) arrived at nearly similar results, differing, however, in this, that he found the coagulation of albumen to take place almost constantly at the *negative* electrode; on which circumstance, at the suggestion of the late Sir Humphry (then Mr.) Davy, he founded a theory of the coagulation of albumen. According to Mr. Brande albumen owes its solubility in water solely to the presence of soda; and that in consequence of the *separation* of this alkali at the negative electrode, the albumen is there deposited in an insoluble form. Several and important objections might be urged against this hypothesis, and amongst others, that the proportion of soda existing combined with albumen in white of egg or serous fluids is really so minute that it can by no means be considered as the solvent body; moreover, the albumen of egg or serum coagulates, as is well known, by heat, which is not the case with the same substance when combined with soda; added to which, the fact of albumen being precipitated at the positive electrode by a very weak electric power, is at once, I conceive, sufficient to demonstrate the untenable nature of the position assumed by Mr. Brande in 1809, and adopted by later authors.

21. In offering an explanation of the coagulation of uncombined albumen by electric currents of weak tension, I am far from presuming that it is altogether unobjectionable; but still I think that it explains the circumstance of its coagula-

tion in a manner more consistent with known facts than any other I have yet met with.

22. I conceive it will be granted by every one at all acquainted with the true nature of electro-chemical decomposition, that a solution of albumen free from the minutest trace of saline or other admixture, would not be affected by a current of electricity (premising that all sources of error arising from the evolution of heat are avoided) of any degree of tension insufficient for its disintegration and resolution into its ultimate atoms; for, like sulphuric acid, soda, or other *ions* *, when *uncombined* it can have no possible tendency to pass to either electrode; but when combined with an alkali, and playing the part of an *electro-negative* or acid body, and becoming an *anion*, we should expect it to pass to the positive electrode (*anode*); or when acting as a base, becoming a *cation*, to pass to the negative electrode (*cathode*). How then does an electric current effect the coagulation of albumen? Not at all, I believe, by a *primary* action upon it, but solely by secondary effects, and in the following manner. Fluid albumen can *never* be obtained perfectly free from chlorides, for common salt always exists intimately mixed with it; and even if this could be entirely removed, still, as Raspail has shown (*Nouv. Syst. de Chim. Org.*, p. 195), the hydrochlorate of ammonia is always present. Then on submitting fluid and as far as we can obtain it, uncombined albumen to voltaic action (by means of platinum electrodes), the saline matter present is decomposed, and this consisting chiefly of chlorides of the alkaline metals, the chlorine passes to the positive, the bases to the negative electrodes. As soon as the chlorine is set free in the positive cup (when two are used, or in the proximity of the positive electrode when one cup only is employed,) it precipitates the albumen, a very minute portion of chlorine† being sufficient to precipitate a considerable quantity of albumen; whilst the bases in the negative cup combine with the albumen; so that at the termination of every experiment of this kind, chlorine (as indicated by the odour), hydrochloric acid,

* It may not be altogether irrelevant to remind those who may happen to be not very conversant with the researches of Faraday, that by an *ion* is understood a body not itself composed of other *ions*, and having, when uncombined, no tendency to pass to either electrode, being quite indifferent to the passage of the current. Thus iodine, sulphuric acid, chlorine, acetic acid, soda, ammonia, &c. are *ions*. [See Lond. and Edinb. Phil. Mag., vol. v. p. 164.—Edit.]

† Chlorine is undoubtedly the most delicate chemical reagent for the detection of albumen that we possess, and I may remark that to its presence nitro-hydrochloric acid owes its value as a test for albumen (5.).

and coagulated albumen will be found in the positive, and the albuminates of soda and ammonia in the negative cup.

But if the communication is made with the battery by means of copper instead of platinum electrodes, a slight modification becomes necessary, for another agent comes into play in assisting the coagulation of albumen: I refer to the metal forming the electrodes; for on completing the connection with the battery the positive wire oxidizes, and the oxide of copper in the *nascent* (?) state combines with the albumen, forming an insoluble albuminate of copper, which coagulates round the wire; and thus having two causes operating instead of one (attraction of the oxide of copper for albumen as well as the effect of the chlorine), we find the precipitation of albumen to take place far more speedily and in much larger quantity when copper is used instead of platinum to connect the albuminous solution with the battery; and acting on this principle, whenever I have merely the *detection* of albumen in view, I am accustomed to use electrodes of clean copper wire, connected with a little battery of 5 or 6 pairs of plates (18.) excited only by weak brine, which I find to be amply sufficient for the purpose. But whatever may be the intensity of the electric currents, or whatever the metal forming the electrodes, we have a sufficient number of facts to believe that free albumen is never precipitated by the electric current *itself*, but by its *effects* in setting free chlorine, or determining the formation of an oxide for which albumen has considerable affinity.

23. It may now be asked, why in Mr. Brande's experiments (Phil. Trans. *sup. citat.*) the coagulation of albumen took place as constantly at the negative, as in mine at the positive electrode. To this question a ready solution may be afforded, when we recollect that in Mr. Brande's experiments the batteries employed were large, and excited by strong acids, whereby the electric currents developed were in a state of considerable tension; in consequence of which, the albumen, rendered insoluble by the presence of chlorine at the positive electrode, was *mechanically* conveyed by the current of positive electricity towards the negative side of the battery. That the passage of electricity is capable of exciting currents in liquid conductors of *apparently* considerable mechanical intensity the experiments of Sir John Herschel (Phil. Trans. 1824, p. 162,) are more than sufficient to demonstrate; whilst the fact of such currents being sufficiently energetic to transfer minutely divided solids from the positive towards the negative electrode, even when in separate tubes, has been proved by M. Becquerel, (*Traité de l'Electricité*, tom. iii. p. 102,) who observed finely divided clay to be actually driven by the

mechanical power of such currents, excited in liquid conductors by the passage of electricity, out of the positive tube; and hence by diminishing the tension of the electricity developed (as in my experiments), such transfers may be prevented and the coagulated albumen constantly collected in the positive cup.

24. I was next desirous to examine the effects of electric currents on an alkaline albuminate. In this combination the albumen acts as a tolerably energetic electro-negative element, and might consequently be expected to be coagulated at the positive electrode. To determine this, some albuminate of soda was prepared as perfectly neutral as possible (2.), diluted with water, and placed in two glass cups communicating by a bent tube filled with the same fluid; the cups were connected with the battery of 30 pairs of plates (16.) by means of platinum wires: a copious and rapid coagulation ensued in the positive cup as was expected. In this case the deposition of albumen can very fairly be attributed to the action of the electric current; for being combined with a *cation* (soda) it must necessarily act as an *anion*, and hence like all anions be deposited at the positive electrode (*anode*). A very weak current, as that afforded by 5 or 6 pairs of plates, is sufficient to produce this effect. When copper wire is substituted for platina in the formation of the electrodes, the decomposition takes place with equal rapidity, the albumen being deposited *round* the positive wire in the form of fine almost diaphanous tubes (more resembling organized membrane than anything else), which drop from the wire almost as soon as formed, and are nearly, if not quite free from copper; affording a striking contrast to the irregular, massive, green, precipitation of albumen *adhering* to the copper electrode when uncombined albumen is the subject of experiment.

25. Electrolytic action did not yield equally satisfactory results when combinations of albumen with acids were employed, for the evolution of chlorine (which it is next to impossible to avoid) almost always caused the precipitation of albumen in the positive cup; and although I sometimes succeeded in causing the albumen to coagulate in the negative cup, yet it was never sufficiently constant to authorize any opinion on the basic nature of albumen. Subsequently, however, I succeeded in obtaining more satisfactory results, by avoiding the influence of chlorine on albumen in the positive cup: for this purpose I took two glass cups connected by filaments of cotton moistened with brine, and filled one with water (containing just enough common salt to render it sufficiently conducting), which I connected by a platinum wire with the positive, the other

with acetate of albumen, which was connected in a similar manner to the negative side of the battery of 30 pairs* (16.). In an hour a considerable cloud of albumen had appeared in the negative cup; the fluid in the positive was, of course, acid: to this an excess of caustic soda was added, and the whole evaporated to dryness: on the addition of sulphuric acid to the dry residue, an odour of acetic acid was evolved. This proves, I conceive, that albumen may play the part of an electro-negative body; for whatever objections may be urged against my deductions from this experiment, no one, I presume, would imagine acetic acid to be conveyed to the positive cup, unless it was combined as an *anion* with some basic body, which in this case could be nothing but albumen. I must confess, however, that albumen forms much more perfect combinations with bases than with acids, and appears to be more allied to the electro-negative than to the electro-positive bodies, in this respect bearing to the other products of organization a similar relation to that of silica to the inorganic bodies.

44, Seymour-street, Euston-square.

XX. *On the Action of Voltaic Electricity on Iodic Acid.* By ARTHUR CONNELL, *Esq., F.R.S.E.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN your Number for May 1836, (vol. viii. p. 401) I observe a passage in a paper by Mr. E. Solly stating that iodic acid which had been freed from water by keeping it in fusion for a short time, during which one half of it was decomposed by the heat, was found to conduct electricity extremely well whilst in its fused state, but that as the points of fusion and decomposition of this substance by heat are the same, it was impossible to ascertain whether it was decomposed by the electric current or not, although the ebullition was thought to be thus increased; and further, that a solution of iodic acid is an electric conductor, iodine appearing at the negative pole.

From the bearing of these experiments on several interesting points of electro-chemistry, particularly on the supposed connection between atomic constitution and susceptibility of voltaic decomposition, I do not feel inclined to concede to any one the priority in making and in publishing them, al-

* I find 12 pairs of plates to be quite sufficient for the success of this experiment, providing that a longer time (4 or 5 hours) is allowed.

though of course always extremely happy to find the results obtained by others coincide with my own. In a memoir "On the action of voltaic electricity on alcohol, æther, and aqueous substances," read to the Royal Society of Edinburgh in April 1835, and printed and distributed in May of that year amongst various individuals and societies in London and elsewhere, several principles of voltaic decomposition were examined, and amongst others the supposed general law, that electrolytes are composed of a like number of atoms of their constituent elements; and I ventured to state my doubts as to the universality of the application of that law; and as one argument in support of these doubts, I quoted the very experiment above set forth on fused iodic acid. After showing (printed Mem., p. 24.) by means of the volta-electrometer that iodic acid in solution is not directly decomposed by voltaic agency, but that oxygen is liberated at the positive pole in the same definite proportion as from the electric decomposition of water by the same current, whilst iodine separates at the negative pole in virtue of the secondary action of nascent hydrogen, I observed, p. 26, that it was doubtful whether "iodic acid, although it resists decomposition in solution, may not give way in the dry and fused state." Afterwards, p. 35, comes the experiment itself on the fused acid in the following words: "I made some experiments on dry and fused iodic acid, which, from the slight affinity of its constituents, I thought likely to throw light on the subject. A difficulty, however, presented itself, arising from the circumstance that when completely freed from water its points of fusion and decomposition are extremely near one another. I freed it from water by keeping it in a fused state in a tube for a considerable time after a portion of the acid was decomposed, and until water was no longer evolved. The residue was dry and hard, and was immediately transferred to a long and bent narrow tube; where platinum wires connected with the two ends of a battery of 50 pairs of 2-inch plates were brought into contact with it, the before-mentioned galvanometer being also introduced into the circuit. The iodic acid was then heated to fusion by a spirit-lamp, when immediately a considerable, and even permanent deflection of the needle took place. Although it was thus quite manifest that a current passed, it was impossible for me to say with certainty that the acid was decomposed by the voltaic agency, because the heat applied was itself sufficient to cause decomposition and volatilization of iodine on both sides."

Although I still think that this experiment ought to be viewed with the caution expressed in the above passage, I do

not feel less inclined now, than when I made and published it, to regard it as unfavourable to the existence of the supposed general law. The differences between this experiment and that of Dr. Faraday on fused periodide of mercury, are, that in the latter the conduction is stated to be feeble, and no signs of decomposition are visible; whilst in the former the conduction is so marked as to impress the mind with the idea that decomposition by the electric current is going forward. The signs of decomposition are obvious, although undoubtedly in part at least due to heat; and the relative number of the constituent atoms of the substance acted upon are much more unequal. Various other considerations are hostile to the general law referred to, some of which are stated in the above memoir*.

In the hope that you may find a corner for the preceding observations, I remain, Gentlemen,

Your very faithful Servant,

Edinburgh, Nov. 28, 1836. ARTHUR CONNELL.

XXI. *On the Solubility of certain Metallic Oxides and Salts in Muriate and Nitrate of Ammonia. By* R. H. BRETT, *Esq., F.L.S.*†

FROM a notice contained in the Supplement to the December Number of the Philosophical Magazine, (vol. ix. p. 540), of some experiments of M. Vogel on the solubility of the earthy carbonates in muriate of ammonia, corroborated by some experiments on that subject performed by Mr. J. D. Smith, I have been induced to extend the inquiry, for the purpose of ascertaining whether the same salt, as well as the nitrate of ammonia, exerted any solvent action over certain earthy and alkaline salts, which are either insoluble or very sparingly soluble in water, and how far this solvent power extended to the metallic oxides (more ordinarily met with) and their salts insoluble in water. The following are the results:

Salts of Lime.—1. The carbonate and phosphate readily dissolve in cold solutions of the muriate of ammonia as well as the nitrate, the earthy salts being recently precipitated.

* The volume of the Edinburgh Transactions to which the above memoir belongs has only now come out, but besides the above-mentioned distribution of the printed memoir, the Society's abstract of the paper referred to was published a year and a half ago, and in it the experiment on fused iodic acid is particularly described. A different abstract appeared in Professor Jameson's Journal (July 1835), in which the other topics of the paper were treated at greater length, whilst those above referred to were for the sake of brevity omitted.

† Communicated by the Author.

2. The recently precipitated sulphate dissolves even in the cold, less speedily, however, and perhaps less completely than the carbonate or phosphate.

3. The borate and tartrate dissolve even in the cold.

4. The oxalate does not appear to dissolve either in the hot or cold solution.

The nitrate of ammonia acts much in the same way as the muriate, and a portion of these calcareous salts is retained in the solution of the ammoniacal salts, even after the fluid has been at rest for some time.

Barytic Salts.—1. The carbonate, phosphate, and oxalate undergo solution in a cold solution of muriate of ammonia.

2. The sulphate does not dissolve.

3. The borate and tartrate undergo solution in the cold salt. A solution of nitrate of ammonia appears to dissolve less of the phosphate of baryta than the muriate of ammonia: it acts much in the same way towards the other barytic salts.

Strontian Salts.—1. The carbonate and phosphate are easily dissolved in a cold solution of muriate of ammonia.

2. The oxalate dissolves in a hot solution of the ammoniacal salt.

3. The sulphate does not undergo solution.

4. The borate and tartrate readily dissolve.

The solution of nitrate of ammonia appears to exert a greater solvent action over the oxalate of strontia than the muriate of ammonia does; in other respects it does not differ from the latter salt.

Magnesian Salts.—1. The phosphate of magnesia and ammoniaco-magnesian phosphate dissolve in a hot solution of muriate of ammonia.

2. The carbonate and tartrate dissolve in a solution of muriate of ammonia.

The nitrate of ammonia appears to exert a less energetic solvent action than the muriate.

Salts of Lead.—1. Carbonate of lead is dissolved in muriate of ammonia, especially when the solution of the ammoniacal salt is heated: when the carbonate is in small quantity it is dissolved without heat.

2. The oxide of the same metal is also dissolved, but it requires a longer continuance of the heat.

3. The sulphate when in small quantity is dissolved without heat.

4. The oxalate is dissolved, especially in the warm ammoniacal salt.

5. The tartrate and phosphate are dissolved in the cold solution.

6. The ferro-cyanate and chromate are not dissolved.

7. The iodide is dissolved even in the cold.

Salts of Zinc.—1. The carbonate is dissolved even in a cold solution of muriate of ammonia.

2. The phosphate undergoes solution in the hot salt, as does also the oxide.

3. The oxalate is soluble in a hot solution of muriate of ammonia.

4. The ferro-cyanate of zinc does not appear to dissolve.

The nitrate of ammonia is a somewhat less perfect solvent than the muriate.

Per-salts of Mercury.—1. The oxide is dissolved by a solution of muriate of ammonia, especially when heat is applied.

2. The triple salt produced by adding ammonia to a persalt of mercury is dissolved in the hot solution of the ammoniacal salt.

3. The carbonate is dissolved by a hot solution of muriate of ammonia.

4. The phosphate and oxalate are dissolved in the cold solution of the ammoniacal salt.

5. The periodide is speedily dissolved in a lukewarm solution of muriate of ammonia.

6. The chromate is dissolved in the warm solution.

The nitrate of ammonia acts in the same way as the muriate.

Proto-salts of Mercury.—1. The sub-proto-nitrate does not appear to undergo solution.

2. The black oxide undergoes solution.

3. The chloride, iodide, carbonate, phosphate, and tartrate dissolve in hot or warm muriate of ammonia, less completely, however, than the persalts.

Nitrate of ammonia is not so good a solvent as the muriate.

Proto-salts of Iron.—1. Neither the oxide, carbonate, phosphate, nor prussiate seems to undergo solution in muriate of ammonia or in the nitrate.

Per-salts of Iron.—1. The peroxide and its salts are similarly situated with the protosalts.

Salts of Antimony.—1. The protoxide dissolves in a cold solution of muriate of ammonia.

2. The carbonate dissolves in the hot solution.

3. The prussiate does not appear to suffer solution. The nitrate of ammonia acts in the same way as the muriate.

Salts of Silver.—1. Chloride of silver readily dissolves in hot muriate of ammonia, from which solution muriatic acid does not throw it down.

2. The carbonate undergoes solution in the hot salt.

3. The phosphate and oxalate undergo solution.

4. The prussiate does not appear to undergo solution.

Nitrate of ammonia acts as a very imperfect solvent of the above salts.

Proto-salts of Tin.—1. The oxide appears to be only sparingly soluble in the hot as well as cold solution of muriate of ammonia.

2. The phosphate and prussiate do not appear to dissolve.

3. The oxalate dissolves readily in a warm solution of muriate of ammonia.

The nitrate of ammonia appears to exert a solvent action only on the oxalate.

Per-salts of Tin.—1. Do not appear to undergo solution readily, if at all, either in the muriate or nitrate of ammonia.

Salts of Bismuth.—1. The oxide and carbonate undergo solution in muriate of ammonia.

2. The phosphate and subnitrate readily dissolve.

The nitrate of ammonia exerts no appreciable solvent action over the above bismuthic salts.

Per-salts of Copper.—1. The oxide dissolves, as does also the carbonate, forming a fine deep-blue-coloured solution; if, however, the latter be acid, or heat be applied, a green-coloured solution of the chloride or subchloride is formed.

2. The phosphate, oxalate, and prussiate do not dissolve.

Nitrate of ammonia does not dissolve the last three salts; it dissolves, however, the oxide and carbonate.

Salts of Manganese.—1. The oxide readily dissolves even in a cold solution of muriate of ammonia; not so the carbonate. When the solution of the oxide is heated it is not precipitated.

2. The phosphate is dissolved in the cold salt, but throws down a portion.

3. The prussiate is not dissolved.

The nitrate of ammonia dissolves the oxide in the cold, not so the carbonate. The phosphate is partially dissolved; heat however, causing it to come down again.

Salts of Cobalt.—1. The oxide is dissolved, even in a cold solution of muriate of ammonia, forming a pink solution; if, however the blue and hydrated oxide of cobalt be heated previously to the addition of muriate of ammonia, so as partially to convert it into a brown colour, the ammoniacal salt does not dissolve the brown portion of oxide.

2. The carbonate is dissolved even in the cold.

3. The phosphate undergoes a less perfect solution, and the prussiate does not dissolve.

If prussiate of potash, which produces a green precipitate in salts of cobalt, be added to a solution of such salts in mu-

riate of ammonia, a yellowish brown-coloured precipitate ensues. The nitrate of ammonia acts much in the same way as the muriate.

Salts of Cadmium.—1. The oxide and carbonate dissolve in a cold solution of muriate of ammonia.

2. The phosphate and oxalate also dissolve in the cold fluid.

3. The prussiate does not undergo solution.

The nitrate of ammonia is a less perfect solvent of the salts of cadmium than the muriate.

Salts of Platinum.—1. The triple compound, chloride of platinum and potassium, is soluble in muriate of ammonia, as is also the chloride of platinum and ammonia.

None of the sulphurets of the preceding metals undergo solution in muriate or nitrate of ammonia. In all the experiments the ammoniacal salts were added to the recently precipitated oxides and salts. The solution of phosphate of lime in muriate of ammonia may, however, be nearly if not entirely precipitated by an excess of caustic ammonia: if, on the contrary, only a small quantity of the caustic alkali be added, although a slight precipitate will take place and the fluid be slightly alkaline, still when filtered it will be found to contain lime by the addition of oxalate of ammonia. In the analysis, therefore, of a solution in muriatic acid supposed to contain phosphate and carbonate of lime, if sufficient ammonia be not added over and above that which is necessary to render the fluid somewhat alkaline, the filtered solution will be precipitated by oxalate of ammonia, and might lead to the supposition that carbonate of lime was present: if even an excess of ammonia had been employed and the fluid heated, the same source of fallacy would exist, because the excess of ammonia would have been driven off, and the resulting fluid would contain phosphate of lime in solution; the same applies to the phosphate of baryta and strontia, also to the phosphate of magnesia.

The salts of lead which are soluble in muriate of ammonia, are precipitated from that solution by an excess of caustic ammonia: hence in precipitating solutions of lead by sulphuric or oxalic acid, where an ammoniacal salt exists, care should be taken that the fluid be strongly alkaline, so as to counteract the solvent power of the ammoniacal salt; the presence of lead in such solutions would, however, be readily detected by sulphuretted hydrogen or hydro-sulphuret of ammonia. The fact of the oxides of iron not being soluble in muriate of ammonia might afford a means of separating them from several other metallic oxides with which they are frequently united, more particularly the peroxide: by this means the latter might be separated from lead, mercury, antimony, zinc, bismuth,

copper, manganese. In certain cases, however, this would not be convenient unless the quantity of the above metals was inconsiderable. The oxides of manganese and cobalt, from their ready solubility, might easily be thus separated from iron.

R. H. Brett.

XXII. *Characters of a new Genus and some undescribed Species of* Araneidæ. *By* John Blackwall, *Esq., F.L.S., &c.**

Tribe, Inequitelæ, Latreille.
Genus, *Deletrix.*

Eyes six in number, unequal in size, aggregated in pairs on the anterior part of the cephalothorax; two pairs are placed laterally, their anterior eyes being the largest, and their posterior ones the smallest of the six; the third pair is intermediate, the eyes which constitute it being oval, and contiguous; the entire group forms two triangles united by the apices composed of the oval-shaped eyes.

Maxillæ enlarged at the base, externally, where the palpi are inserted, and slightly so at the extremity; inclined towards the lip.

Lip short, triangular, and pointed.

Legs long, and moderately robust; the fourth pair is the longest, then the first, which a little exceeds the second in length, the third pair is the shortest. Tarsi triarticulate, the terminal joint being very short and slender.

Deletrix exilis.

This minute spider has the cephalothorax oval, glossy, convex, pointed before, abruptly sloping behind. Mandibles slender, conical, vertical. Pectus broad, heart-shaped, convex, provided with some scattered hairs. These parts, with the maxillæ and lip, are pale red. Legs supplied with hairs, and with two rows of spines on the inferior surface of the tibiæ and tarsi, directed down the joints, those on the first and second pairs being the most conspicuous; they are of a pale yellowish brown colour, the thighs having a tinge of red. The palpi resemble the legs in colour, and are abundantly supplied with long hairs, particularly at the extremity. Tarsi triarticulate; the terminal joint, which is very short and slender, has two curved, pectinated claws at its extremity, and a small brush beneath them. Abdomen oval, thinly covered with hair, projecting over the base of the cephalothorax; its colour is yellowish brown, with a band extending along each side of the medial line on the upper part, some irregular, oblique stripes on the sides, and a longitudinal band occupying the middle of the under part, of a browner hue. Plates of the spiracles pale yellow. Spinning mammulæ prominent, and of a pale yellowish white colour.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{16}$th of an inch; length of the cephalothorax $\frac{1}{32}$; breadth $\frac{1}{45}$; breadth of the abdomen $\frac{1}{36}$; length of a posterior leg $\frac{1}{11}$; length of a leg of the third pair $\frac{1}{14}$.

All the specimens of this spider which I have captured were females, and were discovered under stones and blocks of wood, at Crumpsall Hall, in September, 1836. Being ignorant of the œconomy of the species, I have placed it, provisionally, in the tribe *Inequitelæ*, as it bears a close resemblance to the spiders belonging to the genus *Scytodes* in the structure of the mouth.

* Communicated by the Author.

Genus, *Theridion*, Walckenaër.

Theridion formosum.

Cephalothorax oval, convex, glossy, with a large indentation in the medial line of the posterior region; its colour is yellowish brown tinged with green, a broad black band extending along the middle, and a fine line of the same hue occurring on the margins. Mandibles conical, vertical, armed with a few teeth on the inner surface, near the extremity. Maxillæ inclined towards the lip, obliquely truncated at the extremity, on the outer side. These parts are of a reddish brown colour, the lip, which is nearly semicircular, being black at the base. Pectus heart-shaped, with small prominences on the sides, opposite the legs; it is yellowish brown in the middle, and has broad, black, lateral margins, which meet at the point. Legs long, provided with hairs, of a yellowish brown colour, with reddish brown bands; the first pair is the longest, then the fourth, which is a very little longer than the second, the third pair being the shortest. Each tarsus is terminated by three claws; the two superior ones are curved and pectinated, and the inferior one is inflected near its base. The palpi resemble the legs in colour, and have a curved, pectinated claw at the extremity. Eyes nearly equal in size; four, which are intermediate, form a square, the other four are disposed in pairs on the sides of the square, the eyes constituting each pair being placed on a small eminence, and contiguous. Abdomen oval, convex above, projecting greatly over the base of the cephalothorax, thinly covered with hair; along the middle of the upper part extends a broad, dentated, dark red-brown band, bordered anteriorly with pale yellow; the colour of the sides is a lighter shade of red-brown; the under part is yellow, with a tinge of green, a broad, longitudinal, dark red-brown band occupying the medial line. A small, pale, curved, prominent process is connected with the sexual organs. Plates of the spiracles yellow.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{8}$th of an inch; length of the cephalothorax $\frac{1}{16}$; breadth $\frac{1}{24}$; breadth of the abdomen $\frac{1}{16}$; length of an anterior leg $\frac{5}{24}$; length of a leg of the third pair $\frac{1}{8}$.

The male is rather smaller than the female, which it closely resembles in colour; the absolute length of its legs, however, is greater, an anterior one measuring $\frac{13}{48}$ths of an inch; their relative length also is different, the second pair being longer than the fourth. The third and fourth joints of the palpi are short; the latter, which is the stronger, projects an acute apophysis from the lower extremity, underneath, and has a protuberance on the outer side, from which proceed several long bristles; the fifth joint is of an oblong oval shape, convex and hairy externally, concave within, comprising the palpal organs; they are moderately developed, complicated in structure, having an obtuse projecting process in contact with the protuberance on the outer side of the fourth joint, and two fine spines at the lower extremity, one of which is curved into a circular form; their colour is dark reddish brown. The terminal joints of the palpi have their convex sides directed towards each other.

I discovered this spider on rails and gates, at Oakland, in June, 1836. The structure of its snare is similar to that of the snares of the other species of *Theridia*.

Tribe, Orbitelæ, Latreille.

Genus, *Epeïra*, Walckenaër.

Epeïra nubila.

Cephalothorax of the male inversely heart-shaped, convex, glossy, with a large indentation in the medial line of the posterior region; its colour

is yellowish brown, with a broad band of brownish black, whose margins are the darkest, extending along the middle. Mandibles conical and vertical. The maxillæ incline towards the lip, which is semicircular, and appear to be somewhat pointed, being obliquely truncated at the extremity. These parts are of a pale reddish brown colour. Pectus heart-shaped, of a yellowish brown hue. Eyes nearly equal in size; four, which are intermediate, form a square, the two anterior ones being placed on a projection of the cephalothorax; the other four are disposed in pairs on the sides of the square, the eyes constituting each pair being contiguous, and placed on a tubercle. Legs moderately robust, clad with hair, of a yellowish brown colour; the first pair is the longest, then the second, the third pair being the shortest. The two superior tarsal claws are curved, and pectinated, and the inferior one is inflected near its base. The palpi resemble the legs in colour, with the exception of the terminal joint, which is dark brown; the third and fourth joints are short, and strong, the latter being most prominent on the outer side; the fifth is oval, convex, and hairy externally, concave within, comprising the palpal organs, which are highly developed, and are provided, on the outer side, with a strong spine curved into a circular form, and terminating in an obtuse projection at their lower extremity; they are of a dark red-brown colour. The terminal joints of the palpi have their convex sides directed towards each other. Abdomen oval, convex above, thinly covered with hair, projecting over the base of the cephalothorax; the upper part is brownish black; the sides and under part are pale yellow, the former being almost white at their anterior part, near the cephalothorax. The plates of the spiracles are yellow, and each is surrounded by an irregular black line.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{13}$th of an inch; length of the cephalothorax $\frac{1}{28}$; breadth $\frac{1}{32}$; breadth of the abdomen $\frac{1}{32}$; length of an anterior leg $\frac{1}{9}$; length of a leg of the third pair $\frac{1}{15}$.

I discovered males of this species on rails and gates, at Oakland, in June, 1836.

Tribe, Laterigradæ, Latreille.
Genus, *Philodromus*, Walckenaër.

Philodromus variatus.

Cephalothorax broad, convex, hairy, inversely heart-shaped, with a minute indentation in the medial line of the posterior region; sides dark-brown mingled with yellow-brown, a broad band of the latter hue extending along the middle. Mandibles moderately strong, conical, vertical, armed with one or two small teeth on the inner surface, of a reddish brown colour. Maxillæ yellowish brown, gibbous underneath, at the base, inclined towards the lip, which is triangular, rounded at the tip, and of a dark-brown colour, being palest at the apex. Pectus heart-shaped, thinly covered with whitish hairs, of a yellowish brown colour, with reddish brown lateral margins. Legs long, slender, provided with hairs and sessile spines, of a pale reddish brown hue, which is deepest at the joints; the second pair is considerably the longest, then the first, the third pair being slightly longer than the fourth. Each tarsus is terminated by two curved, pectinated claws, beneath which is a small brush. The palpi resemble the legs in colour, and have a curved, pectinated claw at their extremity. Eyes disposed in two transverse, curved rows, in the form of a crescent, on the anterior part of the cephalothorax; the anterior row is much the shorter, and the lateral eyes of the posterior row, which are seated on a small eminence, are the largest of the eight. Abdomen oval, hairy, convex above,

projecting a little over the base of the cephalothorax; its colour is yellow-brown, mingled with red-brown, and very dark brown; a band of the latter hue extends from the anterior part, along the middle, nearly half its length; this band is comprised between two parallel bands of pale yellow-brown, and on each side of it are two sunken spots of very dark brown, forming, together, a small quadrangle; about the middle, several oblique, very dark brown patches occur, behind which is a curved, transverse line of the same tint; a black streak passes upwards from each spinning mammula of the superior pair, the space between them being of a glossy yellow-brown colour; the sides are red-brown blended with brownish black; the under part is yellowish white faintly tinged with dull green, a large, longitudinal band of very dark brown occupying the middle. The plates of the spiracles are dull yellow.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{4}$th of an inch; length of the cephalothorax $\frac{1}{11}$; breadth $\frac{1}{11}$; breadth of the abdomen $\frac{1}{9}$; length of a leg of the second pair $\frac{9}{24}$; length of a leg of the fourth pair $\frac{7}{24}$.

I found the female of this species on rails, at Oakland, in August, 1836.

Philodromus mistus.

Cephalothorax broad, convex, hairy, inversely heart-shaped, with a minute indentation in the medial line of the posterior region; it is of a yellowish brown colour, having a broad, longitudinal, dark brown band on each side, and narrow, pale yellowish white margins. Mandibles moderately strong, conical, vertical, armed with one or two very small teeth on the inner surface. Maxillæ powerful, enlarged externally, where the palpi are inserted, very gibbous underneath, at the base, and greatly inclined towards the lip, which they encompass. These organs are of a yellowish brown colour, with the exception of the lip, which is triangular, rounded at the tip, and dark brown at the base, the apex only being yellowish brown. Pectus heart-shaped, thinly covered with whitish hairs, of a yellowish brown colour, freckled with minute, blackish spots, which are densest on the sides. Legs long, slender, provided with hairs and sessile spines, of a pale yellowish brown colour, interspersed with minute blackish spots, which are scarcely perceptible without the aid of a magnifier; the second pair is considerably the longest, then the first, the third pair being slightly longer than the fourth. Each tarsus is terminated by two curved, deeply pectinated claws, beneath which is a small brush. The palpi resemble the legs in colour, and have a curved, pectinated claw at their extremity. Eyes disposed in two transverse curved rows, in the form of a crescent, on the anterior part of the cephalothorax; the anterior row is much the shorter, and the lateral eyes of the posterior row, which are seated on a small eminence, are the largest of the eight. Abdomen oval, hairy, convex above, projecting a little over the base of the cephalothorax; the upper part is of a pale yellowish brown colour, mottled with reddish brown, an obscure oblong oval band of the latter hue extending from the anterior extremity, along the middle, nearly half its length; it is surrounded by an irregular band of dark reddish brown, from which some imperfectly defined streaks pass obliquely upwards, particularly in the posterior region; the sides and under part are dull yellow-white, minutely spotted with reddish brown, three longitudinal bands of red-brown extending along the middle, and meeting in a point near the spinners. Sexual organs of a very dark reddish brown colour. Plates of the spiracles dull yellow, or yellowish brown.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{5}{24}$ths of an inch; length of the cephalothorax $\frac{1}{12}$; breadth $\frac{1}{12}$; breadth of the abdomen $\frac{1}{10}$; length of a leg of the second pair $\frac{1}{3}$; length of a leg of the fourth pair $\frac{1}{4}\frac{3}{8}$.

The male is smaller than the female, but the absolute length of its legs is greater, a leg of the second pair measuring $\frac{5}{17}$ths of an inch; their relative length, however, is the same. Cephalothorax dark brown, with a broad band of a paler hue extending along the middle, and narrow, yellowish white margins. Mandibles, maxillæ, lip, pectus, legs, and palpi, of a deep brown colour, the fifth joint of the palpi, and the base of the lip being much the darkest, and the mandibles having a tinge of red. Abdomen of a very dark brown colour, approaching to black on the upper part, freckled with white, the oblong oval extending along the middle of the anterior part being imperfectly defined by an obscure border of whitish hairs; under part very dark brown, approaching to black, the sides being of a paler brown. Plates of the spiracles dark brown. The third and fourth joints of the palpi are short, the latter projecting three apophyses from its anterior extremity; one, on the under side, is short, strong, and furnished with two pointed prominences; the second, which is longer, and acute, is situated on the outer side; and the third, which is very small, occurs in front: the fifth joint is somewhat oval, being prominent on the outer side; it is convex and hairy externally, concave within, comprising the palpal organs, which are highly developed, not very complicated in structure, with a fine spine on the inner side, curved outwards round their lower extremity; they are of a very dark reddish brown colour. The hairs on the cephalothorax and abdomen of very old males reflect brilliant hues of green and purple when viewed in a strong light.

I found specimens of this spider, which appears to belong to the section *Vigilariæ* of M. Walckenaër, on gates and rails, at Oakland, in June 1836; in which month the female deposits between sixty and seventy spherical eggs of a pale yellow colour, not agglutinated together, in a cocoon of white silk of a slight texture, and lenticular form, whose greatest diameter measures about $\frac{10}{13}$ths of an inch. The cluster of eggs is subglobose, measuring $\frac{1}{6}$ of an inch in diameter.

Tribe, CITIGRADÆ, } Latreille.
Genus, *Lycosa*, }

Lycosa leucophæa.

This fine species has the cephalothorax large, thickly covered with hair, inversely heart-shaped, depressed on the sides, and in the posterior region; its colour is greenish brown, the carina being the darkest, and immediately below each lateral pair of eyes are two imperfectly defined, yellowish white spots. Mandibles powerful, conical, vertical, armed with teeth on the inner surface, clad with grayish hairs in front, and densely fringed with pale red ones near the extremity, on the inner side. Maxillæ strong, straight, enlarged and somewhat rounded at the extremity, which is fringed with pale red hairs on the inner side. Lip nearly quadrate, being rather broader at the base than the apex. These parts are of a very dark brown colour, the maxillæ being paler at their extremities. Pectus heart-shaped, dark brown, covered with hoary hairs. Legs robust, abundantly provided with hairs and sessile spines; they are yellowish brown, with spots and bands of dark brown. The palpi resemble the legs in colour, and are furnished with a slightly curved claw at the extremity. Each tarsus is terminated by two curved, pectinated claws. Eyes eight in number, unequal in size; four, which are minute, form a row in front, the two exterior ones being the smallest of the eight; the other four are placed on the sides of the anterior part of the cephalothorax, and form nearly a square, the anterior pair being the largest of all. Abdomen oval, convex above, projecting over the base of the cephalothorax; it is rather broader at the posterior

than the anterior extremity, and is thickly clad with hair; its colour is grayish brown, with a faint tinge of olive, a broad, obscure, dentated band of a lighter hue, which terminates in three points, extending from the anterior part, along the middle, nearly one half its length, and a series of pale, transverse, curved lines, somewhat enlarged at their extremities, and diminishing in length as they approach the spinners, occupying the space between the termination of the band and the extremity of the abdomen: the under side is of a yellow brown colour.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{3}{5}$ths of an inch; length of the cephalothorax $\frac{3}{10}$; breadth $\frac{1}{4}\frac{1}{8}$; breadth of the abdomen, $\frac{1}{4}\frac{1}{8}$; length of a posterior leg 1 inch; length of a leg of the third pair $\frac{4}{5}$ths.

The male resembles the female, but is rather lighter-coloured, and more distinctly marked. Though somewhat less, its legs are longer, a posterior one measuring 1 inch and $\frac{1}{12}$. The fourth joint of the palpi is a little longer than the third; the fifth is of an oblong oval form, and is thinly covered with dull brown hair; underneath, at the upper extremity, is a small aperture containing the palpal organs, which are neither highly developed, nor complicated in structure, and are of a dark red-brown colour.

I discovered males, females, and young of this species in May, 1836, among water-worn stones and fragments of rock, on the banks of the river Llugwy, near Capel Curig, Caernarvonshire.

Crumpsall Hall, Nov. 9, 1836.

XXIII. *On the Composition of two Rectangular Forces acting on a Point.* *By the Rev.* R. Murphy, *M.A.**

LET two forces, X, Y, act upon a point, which we may suppose the origin of coordinates, and let their directions be those of the axes of x and y respectively.

If the forces X, Y be doubled, trebled, &c., the resultant would then be the sum of the resultants of two or three, &c. systems of forces similar to X and Y; that is, it would be accordingly doubled, trebled, &c., in magnitude, without any change of direction.

Now as X and Y may be regarded as the multiples of very small forces, it follows that the ratio $\frac{\mathrm{Y}}{\mathrm{X}}$ remaining constant, the ratio $\frac{\mathrm{R}}{\mathrm{X}}$ (R being the resultant) and the inclination of R to the axis of x will also remain invariable.

The equation to the line in which the resultant acts is therefore

$$y = f\left(\frac{\mathrm{Y}}{\mathrm{X}}\right) \cdot x \quad . \quad . \quad . \quad . \quad (1.)$$

the form of the function f being at present unknown.

* Communicated by the Author.

Suppose now two equal arbitrary forces, Z and $-$ Z, to be applied at the origin in opposite directions, and perpendicular to the plane of X and Y: it is clear that the resultant will not be influenced by this couple.

Taking the axis of z in the direction of Z, the forces X, Y, Z, $-$ Z may be compounded, the first and third together, giving a resultant which acts in the direction expressed by the equations

$$z = f\left(\frac{Z}{X}\right) \cdot x \text{ and } y = 0;$$

and then the second and fourth, which furnish the equations

$$-z = f\left(\frac{Z}{Y}\right) \cdot y \text{ and } x = 0.$$

Let the plane of these two right lines be expressed by $z = a\,x + b\,y$, and putting successively $y = 0$ $x = 0$, we find

$$a = f\left(\frac{Z}{Y}\right) \quad b = -f\left(\frac{Z}{Y}\right).$$

The actual resultant must be at once in this plane and in that of x, y; its equation is therefore $0 = a\,x + b\,y$ or $y = -\frac{a}{b} \cdot x$, which, compared with equation (1.), gives

$$f\left(\frac{Y}{X}\right) \times f\left(\frac{Z}{Y}\right) = f\left(\frac{Z}{X}\right);$$

or if $\frac{Y}{X} = \alpha$ $\frac{Z}{Y} = \beta$ $f(\alpha) \times f(\beta) = f(\alpha\,\beta)$,

whatever may be α and β; and it is exceedingly easy to show that this equation requires $f(\alpha)$ to be of the form α^m: thus the equation to the resultant is

$$y = \left(\frac{Y}{X}\right)^n \cdot x \quad . \quad . \quad . \quad . \quad (2.)$$

it remains to find n.

Let θ be the angle which the resultant of X and Y makes with the axis of x, and ϕ that which the resultant of X $-$ Y and X + Y makes with the same, the former force X $-$ Y being in the direction of x, and X + Y in that of y; the equation to this resultant is

$$y = \left(\frac{X+Y}{X-Y}\right)^n \cdot x \quad . \quad . \quad . \quad (3.)$$

The latter system of forces may be split into two, namely' first X and Y, which give a resultant R inclined to the axis of x at an angle θ, and, secondly, $-$ Y and X, the former in the direction of the negative axis of x, and the latter in the direction of the positive axis of y; this system, being perfectly similar to the first, will give the same resultant in magnitude inclined to the axis of y at an angle θ, and therefore to the positive axis of x at an angle $\frac{\pi}{2} + \theta$.

These two equal resultants, therefore, include a right angle, which *their* resultant, that is, the resultant of the proposed system, bisects; hence

$$\phi = \frac{\pi}{4} + \theta \quad \text{and} \quad \tan\phi = \frac{1 + \tan}{1 - \tan_{\theta}};$$

and putting for $\tan\phi$ $\tan\theta$ their values deduced from the equations (2.), (3.), we have, whatever may be X or Y,

$$\left(\frac{X + Y}{X - Y}\right)^n = \frac{X^n + Y^n}{X^n - Y^n},$$

which evidently requires that $n = 1$; thus the direction of the resultant is completely known.

With respect to the magnitude, we have seen that $\frac{R}{X}$ remains invariable at the same time with θ, which determines the direction of the resultant.

Now X may be regarded as the resultant of a force R′ in the direction of R, and ρ in a direction perpendicular to R.

R′ and X have the same inclination as X and R, therefore

$$\frac{R'}{X} = \frac{X}{R}.$$

ρ and X have the same inclination as Y and R, therefore

$$\frac{\rho}{X} = \frac{Y}{R}.$$

The components of X are therefore $R' = \frac{X^2}{R}$

$$\rho = \frac{X\,Y}{R}.$$

Similarly Y may be decomposed into R″ in the same direction as R′ and R, and ρ' in a direction perpendicular to R and opposite to the direction of ρ.

P 2

Hence $$R'' = \frac{Y^2}{R}$$

$$\rho' = -\frac{Y\,X}{R}$$

Compounding now the four forces ρ, ρ', R', R'', the first two destroy each other, the latter two give a sum; that is,

$$R = R' + R'' = \frac{X^2 + Y^2}{R} \text{ or } R^2 = X^2 + Y^2.$$

Having proved that $\frac{Y}{X} = \tan(\theta)$ and $R^2 = X^2 + Y^2$, we easily obtain $X = R\cos\theta$, $Y = R\sin\theta$, which contain the principles of the composition and decomposition of any forces acting on a plane upon one point.

2, Bateman's Buildings, Soho, Dec. 23, 1836.

XXIV. *On the Classification of Vegetables.* *By the Rev.* PATRICK KEITH, *F.L.S.*

[Continued from p. 42, and concluded.]

OTHERS have innovated upon Jussieu's nomenclature and general plan of arrangement, but it may be doubted whether they have in this respect improved the system. We will specify only two examples.

I. M. DeCandolle, Professor of Botany at Geneva, an acute and skilful systematist, exhibits the first example of innovation. In his *Théorie Elémentaire* and *Prodromus*, he substitutes the term Exogenæ in place of Dicotyledons, and the term Endogenæ in the place of Monocotyledons and Acotyledons; but we can see no advantage that is gained by the substitution. MM. Desfontaines and Daubenton had already shown that Dicotyledonous plants are exogenous, and Monocotyledonous plants endogenous, and Jussieu was well enough aware of the fact. The new terms, we admit, were not yet imposed; but if exogenous and endogenous are respectively identical in their extent, with Dicotyledonous plants on the one hand and with Monocotyledonous plants on the other, whence could the advantage of the substitution come? And if you extend the meaning of endogenous so as to make it include Acotyledonous plants also, we question the legitimacy of the extension, and contend that their endogeneity is not at all of the same character with that of the Monocotyledons. Besides, the change of terms leaves the affair of method precisely where it was, while it has the effect of keeping Jussieu and his di-

visions too much in the background, as well as of giving room for the remark that the principles of the system are departed from. It is enough if the novel terms are introduced to the aid and illustration of the terms of Jussieu, but not to their entire exclusion. Neither do the names imposed upon the minor divisions seem to us to be any improvement. In what respect is thalamifloræ better than hypopetalæ; or calycifloræ than peripetalæ or epipetalæ? If neither one set of terms nor the other is imposed in strict conformity to the anatomical structure of flowers, why exclude one term that is faulty merely to make room for the introduction of another term that is faulty also? It may be true that the stamens and corolla have always the same insertion; it may be true that, in strictness of anatomical speech, their real insertion is always on the *torus*, but as botanical writers seem satisfied to describe them by their apparent insertion, we are of opinion that, unless some very obvious advantage were to follow from it, the nomenclature and divisions of Jussieu ought not to be disturbed. Finally, the division of Acotyledonous plants into Cryptogamæ and Cellulares does not seem to us to be a sufficiently scientific distribution of the group, because the Cellulares are still, in fact, Cryptogamous, as well as the Cryptogamæ themselves. But his Dichlamydeæ and Monochlamydeæ, and Achlamydeæ, we regard as improvements, as affording a convenient ground of subdivision, and imposing names upon distinctions involved, though not designated by individual terms in the arrangements of Jussieu.

II. A learned Professor of Botany among ourselves, of high talent and reputation, exhibits the second example of innovation. In his Introduction to the Natural System of Botany he sets out with dividing vegetables into two grand groups, which he calls classes, the Vasculares and the Cellulares, or flowering and flowerless plants. The terms vascular and cellular stand sufficiently in opposition to one another to form the ground of a legitimate division; but the feature upon which they rest is not more important than that of cotyledons, or the want of them, and gives them, consequently, no apparent claim to supersede the terms of Jussieu. Besides, the terms employed by the Professor are, perhaps, not altogether so correctly descriptive of their respective groups as those employed by Jussieu. The former are of the same extent with cotyledonous plants, and the latter are presumed to be of the same extent with acotyledonous plants. But it is very well known that this is not the fact, as will appear from the following subdivisions into which the Cellulares are distributed by the Professor himself. 1st, The Vasculares are subdivided into the

Exogenæ and Endogenæ of M. DeCandolle,—terms which are substituted in place of the Dicotyledones and Monocotyledones of Jussieu, though we confess that we cannot see the utility of the substitution. The Exogenæ are next divided into Angiospermæ and Gymnospermæ. The former seems to be of a dimension too unwieldy, as containing the polypetalous, apetalous, and diclinous plants of Jussieu, in no less than 165 orders, together with the monopetalæ, in 61 orders more; while the latter seems to be of a dimension too small, as containing only 2 orders, making it nearly the same thing in practice as if they were all angiospermous still; so that the peculiarities which the subdivision involves, though important in themselves, and founded undoubtedly in nature, do not seem to us to be of any great utility as forming the ground of a systematic arrangement,—at least, without having the larger subdivisions subdivided again, into a sufficient number of groups still smaller. The two main divisions of the Exogenæ are called tribes, and yet the orders belonging to them are called tribes also. If this is a fault it is one that admits of an easy remedy, which, we think, the term family would furnish. The Endogenæ are subdivided into petaloideæ with minor groups, and glumaceæ, a subdivision which presents to our notice nothing exceptionable. 2nd, The Cellulares are subdivided into Filicoideæ, Muscoideæ, and Aphyllæ, which might be a good enough division of the class provided it went by the name of Acotyledonous. But as the Cellulares are presumed to have no vascular system, we do not see how they can be legitimately made to include the Filicoideæ, the very diagnosis of which is that they are "flowerless plants, with a stem, *having a vascular system* and distinct leaves*." The Vasculares are the flowering plants, the Cellulares are the flowerless plants. The antithesis is good in fact, but it can scarcely be said to be good in expression. Flowering and flowerless are not so happily opposed to one another as powerful and powerless are, that is, the participle and the adjective, owing to their grammatical peculiarities, do not form a neat or laudable contrast. We admit that this want of systematic symmetry is but a mere trifle after all, though it ought not to have occurred in the work in question. To evade the objection arising from the vascularity of many of the Cellulares, it has been said that they are furnished merely with ducts, but not with spiral tubes. This may be all quite true, yet what are ducts but vessels?

We do not pretend to give advice to these able and eminent botanits, knowing that nothing short of the experience

* Introduction to Nat. System, p. 310.

of the most profound adept is sufficient to qualify or to entitle any one to do so; neither do we expect from *our* speculative demonstrations a result subversive of *their* practical arrangements. We merely claim the privilege of expressing and recording our sentiments, and of stating what seems to us to be exceptionable in the above novel method; or, at the least, not calculated to facilitate the study of the natural system, or to improve the method of Jussieu, which stands in need of no violent innovations to give it in appearance the preeminence which it possesses in reality. It requires merely a drawing out of the resources which it has within itself, or the addition of such supplementary distinctions as the progress of botanical knowledge may have rendered necessary. In defence of innovations, it has been said that the system, though altered, is still but the system of Jussieu after all. True; for as there is but one system that is natural, and that system Jussieu's, botanists cannot conjure up a new one at their pleasure. "Other foundation can no man lay than that is laid,"—though he may disguise the old one, and build upon it a totally different structure, like Thunberg and Withering in their artificial arrangements. They counted stamens and pistils as did their great master Linnæus, but they mutilated his system and substituted one of their own in its room.

But although we do not approve of the change of nomenclature, or of the innovations upon system introduced, whether by M. DeCandolle or by Dr. Lindley, yet we are very far from wishing to depreciate the merit of their respective works; —works exhibiting such abundant proofs of extensive research, of accurate discrimination, and of just and logical deduction in the tracing of natural affinities, as will enable their respective authors to maintain that high station in the scale of botanical eminence which they had previously reached, and will doubtless secure to them a lasting reputation. If there should be a difficulty in unlocking Dr. Lindley's orders, even with the help of his analytical key, it is to be recollected that Dr. Lindley has never once attempted to disguise or to palliate difficulties, but rather to impress upon the mind of his reader the absolute necessity of unremitted exertion.

Nil sine magno
Vita labore dedit mortalibus.—*Horace, Sat.* 9, *Lib.* I.

His Introduction may not be adapted to the desultory application of the sciolist or trifler, but it will be found to be a very valuable present to the patient and indefatigable student who is content to encounter difficulties, and willing to obtain knowledge at the expense of labour.

If we were called upon to say how it is at all practicable to

adapt the system of Jussieu to the present state of botanical knowledge without innovating upon its principles, in external appearance at least, our reply would be, that availing ourselves of whatever we may find in the works of the above-mentioned authors, or of others, calculated to illustrate the character of the groups, or to give perspicuity to the arrangements of Jussieu, and retaining not merely the foundation but the identical structure which he reared upon it, we would venture to add to it a trifle more of extension, or of filling up, in the style and manner, as much as may be, of the original edifice, that the masterly traits of the hand of the founder may never be lost sight of. It will be seen that this adaptation can descend no lower than to the distribution of classes. The orders and their arrangement will be continually changing as long as there shall remain new plants to be collected or new affinities to be discovered, but we do not see the necessity of any violent alteration in the circumscribing of the larger groups. All that we regard as necessary is comprised in the following tabular sketch, giving, as we fancy, a neatness of outline to the higher divisions of the system, by the formal introduction of a very few distinctions that were either implied in it from the beginning or rendered necessary by the progress of analytical research.

Vegetables.

Group I. COTYLEDONOUS PLANTS.—Vascular with spiral tubes;—phænogamous, — bisexual, — angiospermous.

Divis. I. Dicotyledons.—Growth exogenous,—circumferential.

Subd. I. *Dichlamydeæ.*—Floral envelope double,—a calyx and corolla.

Sect. I. *Polypetalous.*

Class I. Hypopetalæ.
II. Peripetalæ.
III. Epipetalæ.

Sect. II. *Monopetalous.*

Class IV. Hypocorollæ.
V. Pericorollæ.
VI. Epicorollæ.
1. Synantheræ.
2. Corisantheræ.

Subd. II. *Monochlamydeæ.*—Floral envelope single,—a perianth or presumed calyx.

Sect. I. *Apetalous.*

Class VII. Hypostamineæ.

VIII. Peristamineæ.
IX. Epistamineæ.
Sect. II. *Anomalous.*
Class X. Diclines.
1. Angiospermæ.
2. Gymnospermæ.

Divis. II. Monocotyledons.—Growth endogenous,—central. Floral envelope a perianth, often in two rows; sepaloid, petaloid, or glumaceous.

Class XI. Monohypogynæ.
XII. Monoperigynæ.
XIII. Monoepigynæ.

Group II. ACOTYLEDONOUS PLANTS. — Cellular, or, if vascular, without spiral tubes?—Cryptogamous.

Class XIV. Ductulosæ.—Cellular, with interspersed ducts,—seminiferous.
XV. Eductulosæ.—Wholly cellular;—gemmiferous.

Thus the whole of the vegetable kingdom is divided into two grand groups, without any sacrifice of the technical language of Jussieu. For although his system does not actually exhibit a division into Cotyledonous and Acotyledonous plants, yet it evidently and essentially involves that distinction. Hence the introduction of the former term is only the completing of the contrast which was already implied in the use of the latter. We have thought it right to put the group designated by the positive term first, because the student cannot be supposed to know well what is meant by an acotyledonous plant till he has already found out what is meant by a cotyledonous one; and although the term acotyledonous is negative in its composition, yet the character which it points out to the learner is positive, namely, that of the want or absence of cotyledons; so that the division is legitimate in whatever aspect you survey it. But we do not rest content merely with a correct antithesis. We avail ourselves of all the lights, old and new, that have been thrown upon either group. In the former we recognise its vascular, its phænogamous, its bisexual, and its angiospermous characters; and in the latter, its cellular, but partially vascular and cryptogamous characters. We accept them as auxiliaries illustrative of the respective groups, but we do not discard the old terms of Jussieu merely that we may use them as synonyms to new ones of our own.

The first grand group Jussieu distributed into two divisions, dicotyledonous and monocotyledonous—divisions that are well contrasted and cannot be improved. All that we add

is merely their exogenous and endogenous characters respectively.

The division of the Dicotyledons we subdivide into Dichlamydeæ and Monochlamydeæ, terms invented by DeCandolle, but not introducing any new principle that was not already to be found in the system, and in full and actual operation. For although the terms were not there, the things signified by them were there, and were made available to the purposes of arrangement, though not designated by individual names. The former term is equivalent to the double envelope of calyx and corolla, and the latter to the single envelope or perianth. They are all exogenous.

The division of the Monocotyledons Jussieu did not subdivide into any minor groups, and neither do we. We notice merely the leading peculiarities of the perianth. They are all endogenous.

In the whole of the above divisions, or subdivisions, the classes are uniformly founded on the mode of the insertion of the stamens, as being hypogynous, perigynous, or epigynous. For although a novice might fancy that the principle of arrangement changes with the change of termination in the names that have been imposed upon the classes, yet the more experienced botanist knows that the origin of the stamens and corolla is uniformly and universally the same, and that whatever is predicable of the one in that respect, is predicable also of the other; and although the introduction of these distinctions, at least in the circumscribing of classes, has been denounced as being wholly and essentially artificial, as well as utterly and absolutely extravagant, on the score of its exhibiting a want of due œconomy in the husbanding of resources, and an improvident expenditure of botanical ammunition that might have been rendered available in the construction of orders and of genera *, yet this want of due œconomy is altogether imaginary. There is an abundance of characters remaining for the constructing of orders and of genera, as resulting from other and important peculiarities discoverable in the form, structure, or position of the stamens, pistils, ovary and ovula, fruit, seed, embryo, or from additional and similar peculiarities discoverable in other parts of the flower or plant. Hence the distinctions founded on the mode of insertion, instead of being an objection to the method of Jussieu, are only a proof of its excellence, in the facility which they give to investigation and in their applicability to the whole of the grand group of cotyledonous plants, whether dicotyledonous or mo-

* Roscoe on Arrangements, Linn. Trans., vol. xi. part I. [Or Phil. Mag. and Annals, N. S., vol. vii.]

nocotyledonous, dichlamydeous or monochlamydeous, polypetalous, monopetalous, or apetalous; so that by retaining the terms and divisions of Jussieu, we are, as it were, always in company with him, or meeting with him at every turn. Hence also the plan of procedure and the inquiries to be made by the student are always the same in all the divisions of the group. Is the plant cotyledonous or acotyledonous; is it dicotyledonous or monocotyledonous; is its floral envelope single or double; is the flower polypetalous, monopetalous, apetalous, or anomalous; are the stamens hypogynous, perigynous, or epigynous? This analysis brings him down to the several classes of the first grand group, which, from their number, are prevented from being surcharged with too many tribes or families. When botanists are prepared to introduce classes founded upon the principle suggested by Dr. Brown in his Botany of Congo and of Terra Australis, that is, the principle of combining into an aggregate group, to be called a class, such orders as are very closely allied, not merely by a single trait, but by the sum of their affinities, enabling us to dispense with the use of empirical characters entirely, then it will be time enough to discard the classes of Jussieu. The objections to which they are liable apply with equal force to the divisions by which they have been superseded in the works of the above systematists; all of them being clogged with anomalies that will puzzle the learner and impede him in his career, let him embrace what system he will.

The second grand group Jussieu did not divide into any minor groups, but introduced merely as a single class. Yet there is an evident demand for such a division, both from the number of species which the group contains and from the peculiarities of structure which several of its tribes display. We adopt a division founded upon anatomical principles, and indicated by features sufficiently obvious, as well as designated by terms which, though novel, are peculiarly appropriate, namely, the *Ductulosæ*, or cellular plants with ducts, but without spiral tubes, as it is said; and the *Eductulosæ*, or plants wholly cellular; the former propagated, perhaps, by seeds, the latter by gems or sporules. The above terms appear to have been originally introduced by Mr. Arnott, of Edinburgh, and seem to us to be quite unexceptionable *. The groups which they designate we would erect into classes, the number being still 15; for though we have thus split one of Jussieu's classes into two, for reasons that to us seem valid, we have elsewhere run two of his classes into one—the Epicorollæ synantheræ, and Epicorollæ corisantheræ, for reasons that seem

* *Encyc. Brit.*, art. BOTANY.

equally valid, by reducing the peculiarities to sections. Beyond this our remarks do not extend. It is the part of the experienced and practical botanist to reduce classes to orders, or to suborders, if necessary, and to construct their diagnosis; or rather, perhaps, by reversing the process and advancing in the line of ascent, to reduce orders or suborders to classes; and to the experienced and practical botanist we are content to commit the task. P. KEITH.

Charing, Kent, Feb. 15, 1835.

XXV. *Researches in Organic Chemistry.—First Series. Contributions to the History of Pyroxylic Spirit and of its derived Combinations.* By ROBERT J. KANE, *M.D., M.R.I.A.*

[Continued from p. 51, and concluded.]

Of the Products of the Oxidation of Pyroxylic Spirit by Sulphuric Acid and Black Oxide of Manganese.

WHEN sulphuric acid, black oxide of manganese, and pyroxylic spirit are brought into contact, the action which ensues is very violent, and there is so much effervescence that unless the retort be taken very large in proportion to the quantity of materials used there is great danger of its boiling over. By previously diluting the sulphuric acid with water, allowing the mixture to cool before adding thereto the black oxide of manganese and sulphuric acid, this can be to a great extent avoided, and the quantity of product is also by this means considerably increased.

The proportions found most advantageous were two ounces of pyroxylic spirit, three of sulphuric acid and three of water, and two of black oxide of manganese. The manganese and pyroxylic spirit having been first put into the retort and afterwards the diluted and cold sulphuric acid added, the whole is to be very gently heated in a water-bath, and as soon as the mass commences to froth, the fire should be withdrawn, in order to prevent the temperature of the water from rising too high, in which case the mixture is liable to boil over. Once commenced, the distillation goes on almost of itself; as the ebullition moderates the temperature can be again raised, and the distillation continued as long as any fluid comes over, by the water-bath. If the water- be then replaced by a sand-bath and the receiver changed, a liquor can be obtained which is a dilute formic acid.

The product obtained by distillation in the water-bath is very heterogeneous; it must be rectified in the water-bath. It begins to boil under 40° centigrade, and the boiling point gra-

dually rises to 70° or 80° centigrade. The liquors of different boiling points must be separated. In the first portions are found some aldehyd, but principally a new and peculiar fluid, to which the name formal can be applied, from considerations which shall hereafter be brought forward. The latter portions, which boil from 60° to 70°, consist of Dumas's pyroxylic spirit, together with small quantities of a fluid identical with that pyroxylic spirit originally analysed by Liebig, and to which I shall again recur.

The formal, obtained pure by rectification over chloride of calcium, is colourless, and of a penetrating but aromatic odour; it mixes with water in every proportion, from which it is separated by the addition of chloride of calcium; the separation is however by no means perfect. Pure formal boils at 38° centigrade. Subjected to analysis,

(No. 1.) 0·502 material gave

Water = 0·428

Carbonic acid = 0·833,

(No. 2.) 0·528 material gave

Water = 0·441

Carbonic acid = 0·875;

from whence follows,

	No. 1.	No. 2.
Carbon	= 45·87	45·82
Hydrogen	= 9·47	9·28
Oxygen	= 44·66	44·90
	100·00	100·00

with which agrees the formula

4 atoms carbon	= 305·89	45·77
10 —— hydrogen	62·39	9·34
3 —— oxygen	300·00	44·89
	668·28	100·00

To confirm this analysis by the density of the vapour the following experiments were made:

(No. 1.) Weight of balloon with air = 37·244 grammes.

—————————— vapour = 37·520 ——

Excess = 0·276 grammes.

Volume of balloon	= 266·5 cub. cent.
Residual air	2·5 cub. cent.
Temperature of air	16·8 cent.
Temperature of vapour	97·75 cent.
Barometer	0·750 metre.

From whence results,

Weight of a litre of vapour at zero centigrade and 0·76 met. bar. press.		= 3·126 gram.
Specific gravity of vapour		4·4065 —
(No. 2.) Weight of balloon with air		49·645
Weight of balloon with vapour		46·938
Excess = 0·293 gram.		
Residual air		= 0
Temperature of air		21° cent.
——— vapour		98 cent.
Barometer		0·75 metre.
Result:	Weight of a litre	3·1279 gram.
	Specific gravity	2·408 —

The theoretical density is

4	volumes gaseous carbon		= 3·3712
10	———	hydrogen	0·6880
3	———	oxygen	3·3078
			3 ÷ 7·3670 = 2·4556

It is curious that the variation from theory in these determinations lies in the opposite direction from what is usual, and the complete coincidence of the two experiments proves that the deviation results not from manipulation but from the nature of the fluid itself. The simultaneous though trivial excess in carbon induces me to attribute both to the presence of a trace of aldehyd, from the last portions of which, owing to the great volatility of the formal, the latter can scarcely be obtained free.

The empirical formula of this body $C_4\ H_{10}\ O_3$ allows of many methods of considering its real nature. The first that naturally occurs is that it was a higher degree of oxidation of æther, a tritoxide of ethyle A E . O_3; but although sensible of the importance of this view, with respect to the alcohol and methylene series, and generally to the theory of organic radicals, I am of opinion that the balance of probability inclines in favour of a different arrangement.

When this fluid is brought into contact with potash or baryta, Dumas's pyroxylic spirit is formed, and formate of potash; to illustrate this action we have only to consider that the composition of this fluid is $C_4\ H_{10}\ O_3$, or

$$\begin{array}{ll} \text{3 atoms methylene æther} & = C_6\ H_{18}\ O_3 \\ \text{1 atom formic acid} & = C_2\ H_2\ O_3 \\ \hline & \ C_8\ H_{20}\ O_6 \end{array}$$

Dumas and Peligot had already proved formic acid to be the product of the highest degree of oxidation of pyroxylic

spirit, bearing the same relation to the methylene that acetic acid bears to the alcohol series. In fact, the fluid under examination is the acetal of the methylene series, and is formed under perfectly similar circumstances; the pyroxylic spirit is by the sulphuric acid converted into methylene æther, and

4 atoms methylene æther	$=$	$C_8\,H_{24}\,O_4$
lose 4 atoms of hydrogen	$-$	H_4
and gain 2 atoms of oxygen	$+$	O_2
		$C_8\,H_{20}\,O_6$

an atom of the hence-denominated formal.

As acetal can likewise be considered as being

$$\left.\begin{array}{ll} \text{1 atom æther} & C_4\,H_{10}\,O \\ \text{1 atom aldehyd} & C_4\,H_8\,O_2 \end{array}\right\} C_8\,H_{18}\,O_3,$$

so can formal be considered as consisting of

$$\left.\begin{array}{ll} \text{1 atom methylene æther} & C_2\,H_6\,O \\ \text{1 atom methylene aldehyd} & C_2\,H_4\,O_2 \end{array}\right\} C_4\,H_{10}\,O_3.$$

To these considerations I shall have occasion to advert hereafter.

Of the Pyroxylic Spirit analysed by Liebig.—The composition of formal standing so closely in connexion with the result obtained by Professor Liebig in his analysis of pyroxylic spirit, rendered a further examination of this fluid highly interesting, and the more so as Dumas, in the memoir on the methylene combinations, expresses his inability to account for the difference between their results, which, as was to be expected, resulted from the two chemists having examined two completely distinct bodies.

For the purpose of a new analysis, Professor Liebig was kind enough to put into my hands a small quantity of his original pyroxylic spirit which had remained in his cabinet of preparations. It was purified from some water and a brownish colouring matter, by rectification over chloride of calcium, and possessed the boiling point and other properties which that celebrated chemist had assigned to it. On analysis, 0·470 of material gave

$$\begin{array}{ll} \text{Water} & = 0{\cdot}477 \\ \text{Carbonic acid} & = 0{\cdot}933, \end{array}$$

from whence results the composition

$$\left.\begin{array}{ll} \text{Carbon} & = 54{\cdot}88 \\ \text{Hydrogen} & = 11{\cdot}27 \\ \text{Oxygen} & = 33{\cdot}85 \end{array}\right\} 100{\cdot}00.$$

Liebig's former result was		Theory gives	
Carbon $=54{\cdot}75$	100	4 atoms Carbon $\propto 53{\cdot}83$	100·00
Hydrogen $=10{\cdot}75$		10 — Hydrogen $\propto 10{\cdot}97$	
Oxygen $=34{\cdot}50$		2 — Oxgen $\propto 35{\cdot}20$	

The result allows no other interpretation, although the substance was evidently impure, and unfortunately the quantity remained too small to allow of the necessary operations for obtaining it quite pure. Under such circumstances an accurate determination of the density of the vapour could not be expected; nevertheless, the following experiment was made:

Weight of the balloon with air =	46·372 gram.
——————— vapour	46·505 —
Volume of the balloon...............	285·6 cub. cent.
Residual air	8·0 ——
Temperature of the air	19·6° c.
————— vapour	97·6 c.

Barometer 0·748 metre.

Weight of a litre of vapour	2·3674 gram.
Specific gravity ...(air=1).........	1·824 —

The theoretical composition gives

4 volumes of gaseous carbon =	3·3712
10 ————— hydrogen	0·6880
2 ————— oxygen	2·2052

$$3 \div 6{\cdot}2644 = 2{\cdot}0881.$$

The difference between the calculated and experimental result is here considerable, but yet not so considerable as to cast any doubt on the real composition of the substance.

This fluid presents, in connexion with that previously described, some interesting relations. It is evident that both may be considered as degrees of oxidation of the same radical ethyle A E = $C_4\ H_{10}$; but a still more remarkable connexion results from considering Liebig's pyroxylic spirit as

$$\left.\begin{array}{ll}\text{3 atoms of methylene æther} & = C_6\ H_{18}\ O_3 \\ \text{1 atom of methylene aldehydene} & \ C_2\ H_2\ O\end{array}\right\} C_8\ H_{20}\ O_4.$$

In this point of view it resembles formal, the formic acid of the latter being replaced by a lower degree of oxidation of the same radical, by, in fact, the aldehydene of the methylene series, formed by the subtraction of four atoms of oxygen from methylic æther.

I shall return to the history of the methylene aldehyd in another paper.

The properties and composition of the bodies just described, as well as those of the chloral of Liebig, the chloroform and their congeners, point out the existence of a series which may now be tabulated, although some of its members are as yet known only in a state of combination. I shall therefore conclude this abstract by a *resumé* of these compounds in the form they appear naturally to assume.

Hypothetic radical. *Formyl* $C_2\ H_2$ = Fo.

Oxide of formyl	$= C_2 H_2 O$	$= Fo\, O$
Hydrated oxide / Methylene aldehyd	$C_2 H_4 O_2$	$Fo\, O + H_2 O$
Formic acid	$C_2 H_2 O_3$	$Fo\, O_3$
Chloroform	$C_2 H_2 Cl_6$	$Fo\, Cl_6$
Bromoform	$C_2 H_2 Br_6$	$Fo\, B_6$
Formal	$C_4 H_{10} O_3$	$3\,Mtl.\, O + Fo\, O_3$
Formosal .... / Liebig's pyrox. spirit	$C_4 H_{10} O_2$	$3\,Mtl.\, O + Fo\, O$
Chloral and water ...	$C_4 H_2 O_2 Cl_6 + H_2 O$	
giving chloroform ...	$C_2 H_2 Cl_6$ + formic acid $C_2 H_2 O_3$.	

By the action of water and the chlorine body from pyroxylic spirit, that is, from $C_6 H_4 O_2 Cl_6 + H_2 O$, there should result deutochloride of formyl, $C_3 H_3 Cl_6$, and formous acid, $C_3 H_3 O_3$; but I am not yet satisfied of the composition of the bodies so formed.

XXVI. *Investigation of Formulæ for the Summation of certain Classes of Infinite Series. By* J. R. Young, *Esq., Professor of Mathematics in Belfast College.**

THE general formulæ established in the present paper are, I believe, new; and as they are extensively applicable, and involve but a very moderate amount of numerical labour, they may, perhaps, be found useful on many occasions. The investigation, which is very simple, is as follows:

Since any series of fractions of the form

$$\frac{1}{n(n+p)(n+2p \ldots (n+mp)} \quad \ldots \quad (A.)$$

is equal to $\frac{1}{mp}$, the difference between a series of the form

$$\frac{1}{n(n+p) \ldots [n+(m-1)p]} \quad \ldots \quad (B.)$$

and another of the form

$$\frac{1}{(n+p)\;(n+2p) \ldots (n+mp)} \quad \ldots \quad (C.)$$

it follows that a series of the form A^2 will be equal to $\frac{1}{m^2 p^2}$, the sum of two series of the forms B^2 and C^2, minus $\frac{2}{m^2 p^2}$, a series of the form $B\,C$. Now it is obvious that the two series whose general terms are B^2 and C^2, will, together, be equal

* Communicated by the Author.

to twice the series whose general term is B^2, minus the leading term in the other series; both series being, of course, regarded as commencing at the same value of n. If this value be unit, then, calling the series whose general term is B^2, S′, and that whose general term is A^2, S, we shall evidently have

$$S = \frac{1}{m^2 p^2}\left\{2\,S' - \frac{1}{1^2(1+p)^2(1+2p)^2\ldots[1+(m-1)p]^2}\right\}$$

minus $\frac{2}{m^2 p^2}$, the series whose general term is

$$\frac{1}{n(n+p)^2(n+2p)^2\ldots[n+(m-1)p]^2(n+mp)} \quad . \; . \; (D.)$$

But it is evident, from the original relation

$$A = \frac{1}{mp}(B - C),$$

that the fraction D is equal to

$$\frac{1}{mp}\left\{\frac{1}{n(n+p)^2(n+2p)^2\ldots[n+(m-1)p]^2} - \frac{1}{(n+p)^2(n+2p)^2\ldots[n+(m-1)p]^2(n+mp)}\right\}$$

so that the entire series, whose several terms are generally represented by D, will be equal to $\frac{1}{mp}$, the difference between two series whose corresponding terms are generally expressed by the two fractions within the brackets. If, however, we regard the latter of these series, that is, the subtractive series, to originate at the immediately preceding term, instead of at the term where it actually begins, and then perform the subtraction, we must add to the result the leading term previously introduced; that is, supposing the leading term in each series to have $n = 1$, we must add the fraction

$$\frac{1}{1^2(1+p)^2(1+2p)^2\ldots[1+(m-1)p]}.$$

Now the result of the subtraction spoken of is readily seen to be $-(m-1)p\,S'$; and consequently, by introducing the above correction, the true difference between the two series will be expressed by

$$-\left\{(m-1)p\,S' - \frac{1}{1^2(1+p)^2(1+2p)^2\ldots[1+(m-1)p]}\right\}.$$

It follows, therefore, that the value of the series S must be

equal to the expression marked (1.), together with $\frac{2}{m^3 p^3}$, the expression just deduced, that is,

$$S = \frac{1}{m^2 p^2}\left\{2\, S' - \frac{1}{1^2 (1+p)^2 \dots [1+(m-1)p]^2}\right\} +$$
$$\frac{2}{m^3 p^3}\left\{(m-1)\, p\, S' - \frac{1}{1^2 (1+p)^2 \dots [1+(m-1)p]}\right\}$$

$$\left.\begin{array}{l} \therefore S = \dfrac{4\,m-2}{p^2}\, S' - \dfrac{3\,mp - 2p + 2}{m^3 p^3} \,. \\ \qquad\qquad\qquad \dfrac{1}{1^2 (1+p)^2 \dots [1+(m-1)p]^2} \end{array}\right\} (a.)$$

By this general formula any infinite series of the form

$$\left.\begin{array}{l} S = \dfrac{1}{1^2 (1+p)^2 (1+2p)^2 \dots} \\ \quad + \dfrac{1}{(1+p)^2 (1+2p)^2 (1+3p)^2 \dots} + \&c. \end{array}\right\} \quad . \quad . \quad . \quad (b.)$$

may be readily summoned, provided only that we previously know the summation of the series S′, whose terms differ from those of S by the absence of the final factor in the denominator of each. Knowing, therefore, the value of the series

$$S' = \frac{1}{1^2} + \frac{1}{(1+p)^2} + \frac{1}{(1+2p)^2} + \frac{1}{(1+3p)^2} + \&c. \quad . \quad . \quad (c.)$$

we may, by successive applications of the formula (*a.*), proceed, through all the intermediate summations, up to the summation of (*b.*); and in those cases in which the value of (*c.*) is unknown, and its approximate summation required, we may advantageously commence by first summing a few terms of the more rapid series (*b.*), and then, by means of the formula (*a.*), gradually descend till we arrive at the proposed form S′ = (*c.*).

If in the formula (*a.*) we assume $p = 1$, we have

$$S = \frac{1}{m^2}\left\{\frac{2\,(2\,m-1)}{m}\, S' - \frac{3}{1^2 \,.\, 2^2 \dots m^2}\right\} \quad . \quad . \quad . \quad . \quad (I.)$$

and if $p = 2$,

$$S = \frac{1}{2\,m^3}\left\{(2\,m-1)\, S' - \frac{3\,m-1}{1^2 \,.\, 3^2 \dots (2\,m-1)^2\, 2}\right\} \quad . \quad . \quad (II.)$$

When $m = 0$ the sum of the series in each of these cases is known to be

$$S' = \frac{1}{1^2} + \frac{1}{2^2} + \frac{1}{3^2} + \&c. = \frac{\pi^2}{6}$$

$$S' = \frac{1}{1^2} + \frac{1}{3^2} + \frac{1}{5^2} + \&c. = \frac{\pi^2}{8}\ ;$$

hence by the first formula we have, when

$$m = 1,\ S = \frac{\pi^2}{3} - 3 = \frac{1}{1^2 \,.\, 2^2} + \frac{1}{2^2 \,.\, 3^2} + \&c.$$

$$m = 2,\ S = \frac{\pi^2}{4} - \frac{39}{16} = \frac{1}{1^2 \,.\, 2^2 \,.\, 3^2} + \frac{1}{2^2 \,.\, 3^2 \,.\, 4^2} + \&c.$$

$$m = 3,\ S = \frac{5\,\pi^2}{54} - \frac{197}{216} = \frac{1}{1^2 \,.\, 2^2 \,.\, 3^2 \,.\, 4^2} + \frac{1}{2^2 \,.\, 3^2 \,.\, 4^2 \,.\, 5^2} + \&c.,$$

and so on to any extent.

From the second formula we get, when

$$m = 1,\ S = \frac{\pi^2}{16} - \frac{1}{2} = \frac{1}{1^2 \,.\, 3^2} + \frac{1}{3^2 \,.\, 5^2} + \&c.$$

$$m = 2,\ S = \frac{3\,\pi^2}{256} - \frac{1}{9} = \frac{1}{1^2 \,.\, 3^2 \,.\, 5^2} + \frac{1}{3^2 \,.\, 5^2 \,.\, 7^2} + \&c.$$

$$m = 3,\ S = \frac{5\,\pi^2}{4608} - \frac{43}{4050} = \frac{1}{1^2 \,.\, 3^2 \,.\, 5^2 \,.\, 7^2} + \frac{1}{3^2 \,.\, 5^2 \,.\, 7^2 \,.\, 9^2} + \&c.,$$

and so on as far as we please*.

By means of the general relation

$$A = \frac{1}{m\,p}\,(B - C),$$

a variety of other series may be summed with great ease, all those, for instance, investigated by Mr. Phillips, with the aid of definite integrals, in the Philosophical Magazine for 1832, vol. xi. A single example of the mode of proceeding will be sufficient.

[To be continued.]

XXVII. *On Professor* Müller's *Account of the Reflex Function of the Spinal Marrow. Communicated by* Marshall Hall, *M.D., F.R.S., &c.*

[Continued from p. 57.]

"THE previous considerations however lead us only to the determination of the fact, that, wherever general twitchings originate from local sensation, this takes place by no other

* A valuable paper upon the summation of this class of infinite series, by Mr. Woolhouse, may be seen in the Appendix to the Ladies' Diary for 1836, and another upon the same subject, by Mr. Rutherford, in the Diary for 1837.

connexion of sensorial and motor fibres than through the spinal chord. In very many cases, however, after local excitation of the nerves, there ensue not general, but local twitchings, which, however, must also be constantly explained by the spinal marrow being considered as the connecting link between the sensorial and motor fibres. The cases which may be arranged here, are the following:

"1. The simplest is the case, in which the local sensorial stimulation, propagated to the spinal marrow or brain, excites merely local movements, and these in the parts lying in the neighbourhood, whose motor fibres proceed from the spinal marrow near the sensorial. To these belong the spasms and tremblings of the limbs which are severely burnt, &c. Certain very excitable parts of the organism, as the iris, contract extremely easily, when only slight stimuli excite other sensorial nerves, and the excitement of the latter is propagated to the brain, and from it through the oculo-motor nerve to the short root of the ciliary ganglion, the ciliary nerves, and the iris. It has long been known that the iris is not excitable by light, and that light acts on it only through the medium of the optic nerve and the brain; this results from the experiments of Lambert, Fontana, and Caldani. Rays of light passing through a small cone of paper, or through a small hole in a piece of paper, and thus transmitted through the pupil, and falling on the retina, immediately induce motion in the iris, but have no power when they fall on the iris itself. The iris of an amaurotic eye moreover is immoveable, as long as the sound eye is closed, but contracts when the light excites the optic nerve of the latter. The exceptions in which the optic nerve of the amaurotic eye still retains mobility*, may easily depend on an incomplete amaurosis, or if only one eye was amaurotic, the cause of the motion of the iris in the amaurotic eye was the open state of the sound eye. The mobility or immobility of the iris of an amaurotic eye can and is only to be investigated when the healthy one is closed. Every observation in which this precaution has not been taken is valueless; Van Deen has therefore deceived himself in his otherwise very valuable work†, when having in a rabbit cut away one hemisphere of the brain, and the optic nerve of the same side, he saw the iris contract on the application of a light, and therefore concluded, that the optic nerve had no influence on the iris. For as Van Deen brought the light before both eyes (*ante oculos*), the same result would follow as when the iris of an amaurotic eye is moved by the influence of light on the sound

* See Tiedemann in his *Zeitschrift*, i. p. 252.

† *De Differentia et Nexu inter Nervos Vitæ animalis et organicæ.*

one. Tiedemann's interesting discovery that the arteria centralis retinæ is accompanied by a fine twig from the ciliary ganglion, cannot in this case explain anything. For all vessels are accompanied by nerves; but this twig is distributed with the arteria centralis, and is in no proved connexion with the retina. This reflex action from the brain to the iris takes place through the oculo-motor nerve, which according to Mayo's experiments at every stimulus excites a contraction of the iris*. We know from the same author that the cerebral end of the divided optic nerve, when stimulated, still induces contraction. Thus, in the contraction of the iris there is presented a kind of "Statik" of the excitement between centripetal-sensorial and centrifugal-motor action through the medium of the brain. Other nerves also may alter this *equilibrium*, as the sensorial branches of the trigeminus, for cold water thrown into the nose produces contraction of the iris. To these more simple instances of reflected excitement belongs also the winking of the eyelids from long impression of light, or from a loud sound (what has the optic nerve to do with the auditory?), or from a threatening impression on the sight.

"Further, to these belong the contraction of all the muscles of the perinæum, the sphincter ani, levator ani, bulbo-cavernosus, and ischio-cavernosus in the emission of semen, in consequence of the irritation of the sensitive nerves of the penis; in these cases the spinal marrow is the connecting link between the sensations and motions. Exposed muscles, whose motor nerves are themselves coincidently stimulated in the stimulation of the muscles, do not require these centripetal and centrifugal actions to excite contractions. But the muscles which are covered by sensitive membranes, and do not themselves lie exposed to stimuli, must receive the stimulus to motion through the sensorial excitement of their sensitive covering, the centripetal action of these sensorial nerves, and the centrifugal motor excitement from the brain. Thus the contractions of the glottis and air-passages induced by irrespirable acid gases, cannot result immediately from the excitement of these passages, but from the centripetal-sensorial and centrifugal-motor excitement. Brachet has very fully proved this. For if the nervus vagus on both sides of an animal be divided, an exciting chemical substance introduced into the trachea ceases to act as a stimulus to cough. The cough from stimulus in the air-passages is induced only by sensorial centripetal, and centrifugal motor excitement. It is the same with the contraction of the sphincter ani and sphincter vesicæ

* Magendie's *Journal de Physiologie*, iii. 348.

urinariæ. These muscles cannot themselves be excited to contract by the stimulus of the excrement and urine, but these substances act on the sensitive nerves of the mucous membrane, and excite the spinal marrow, which, constantly charged with motor nervous power, acts back on these muscles; therefore after injury of the spinal marrow the contraction of these muscles ceases.

"2. The second case is, where the sensorial excitement being quite local, the reflex acting excitement from the brain is more diffused, as shown already in the phænomena which accompany cough, in which not only the nervi vagi, but the spinal nerves supplying the thoracic and abdominal muscles, act in coincidence. It is the same with a number of spasmodic respiratory motions, sneezing, hiccup, vomiting, &c., all of which are produced by stimuli of the sensitive nerves of the system of mucous membranes of the respiratory organs and intestinal canal, which stimuli are reflected to the brain, and thence put in action the source of the respiratory motions in the medulla oblongata. I have already in p. 333* mentioned the remarkable peculiarity, that the system of respiratory nerves may be put in action by local stimuli applied to all mucous membranes. For all the motions, cough, sneezing, vomiting, spasmodic involuntary discharge of fæces, involuntary forcible passage of urine, arise from violent irritation of the mucous membranes of the fauces, œsophagus, stomach, intestines, and respiratory apparatus. Sneezing has been explained as a spasmodic affection of the diaphragm; Tiedemann† and Arnold‡ still speak thus of it: however, it has probably nothing to do with the diaphragm, for it is a violent expiration, and the diaphragm is no expiratory muscle, but the contrary. Under the incorrect supposition that sneezing resulted from the diaphragm, the stimulus of the nasal nerves was considered to be propagated to the spheno-palatine ganglion, the Vidian nerve, the sympathetic, the cervical nerves, the phrenic, the accessorius Willisii, and the facial§. The highly talented Tiedemann endeavours also to prove that sneezing does not result from a reflected stimulus from the brain, and supports himself on the fact, that a man has still sneezed from snuff, without any sense of smell. Why should he not, seeing that when the nerves of smell are deficient, the nerves of common sensation in the nose, the nasal nerves, have still as in healthy men the perception of tickling? But by minute anatomy the explanation of a sympathy can still only be

* See note p. 53. † *Zeitschrift*, i. 278.

‡ *Der Kopftheil des Vegetat. Nervensystem*, p. 181.

§ Tiedemann, *l. c.*, p. 278.

attained through the sympathetic nerve. Yet how can sneezing be explained by a connexion of nerves, by which everything and yet indeed nothing can be explained? We may explain anything by it, because the sympathetic is connected with almost all nerves; and yet nothing can be explained by it, because there is not the most remote reason why a stimulus of this nerve of the nose should produce sneezing and not many other motions, as, for instance, an increased motion of the intestinal canal. Nothing can be explained by it, because in no connexion of the sympathetic with another nerve is there an actual union of their filaments. In sneezing, for instance, there is a violent contraction of all the muscles of respiration; all the primitive filaments of the intercostal nerves therefore, which produce contraction of the thoracic and abdominal muscles, must therein be irritated. But how could all these filaments be irritated from the sympathetic nerve, which adds to each of these nerves a fasciculus of filaments, that, far from uniting its primitive filaments with all the primitive filaments of a spinal nerve, only receives them with the latter from the spinal marrow? Now since primitive filaments cannot impart anything to others lying near them, especially in a motor root without a ganglion, so in this case the sympathetic affection of all the primitive filaments of an intercostal nerve by the sympathetic nerve is a perfect impossibility. All these sympathies of sneezing, coughing, vomiting, are done away with, as soon as we know of the reflex function of the spinal marrow and brain, which we have before proved; and no further difficulty lies in the way of the explanation, as soon as one proceeds from the fact, that all respiratory nerves, the facial, vagus, accessorius, phrenic, and the other spinal respiratory nerves of the trunk, by their origin from the medulla oblongata, or their dependence on it, may be easily excited to convulsive motions in muscles, by all stimuli, which are conducted from the sensitive nerves of the mucous membranes to the spinal marrow or the medulla oblongata.

"On every violent stimulus in the intestines, the urinary bladder, and the uterus, contraction of the diaphragm and the abdominal muscles easily ensues, lessening the cavity of the abdomen; and its contents are forced upwards when contained in the stomach (vomiting), or downwards through the rectum or urinary apparatus, or through the genitals as in parturition. The forcible expulsion of fæces is the same phænomenon in the lower part of the intestinal canal as vomiting is in the upper. The forcible expulsion of urine presents the same motions in mental passions; parturition calls into action the same muscles as produce expulsion upwards in vo-

miting; the parturition too which takes place even after death, just like the firm application of the pharynx round a finger introduced into it in a beheaded young animal, shows us, of what important influence, and how intimately connected with life, this power of the spinal marrow is, of being excited to motorial discharges by local excitations of its sensitive (or perceptive) nerves. In many of the stimulations belonging to this class, in vomiting, &c., the sympathetic nerve may indeed take some part, but it is nothing more than that of reflecting, like all other sensitive nerves, the stimulation to the sensorium. But that it may have this action may be shown by an experiment on rabbits; for instance, by tearing the splanchnic nerve in the abdomen, I have observed frequent twitchings of the abdominal muscles, and have repeatedly seen the same phænomenon in other rabbits, though the same experiments did not succeed with me in dogs.

"3. In the cases mentioned under 2, the reflected motion is the motion following on perception, and diffused through a large series of nerves, the respiratory nerves, and it arises most easily from stimulation of the mucous membranes; but in greater stimulation the diffusion of the reflected motions may be still greater, and affect nearly all the nerves of the trunk, when the irritable condition of the spinal marrow is extensive. Among these cases are to be reckoned those of sporadic cholera, (I do not mention Asiatic cholera because of the obscurity of that disease,) in which, when severe, spasms may take place even in the trunk.

"4. In the reflected motions which arise from violent perceptions of the cutaneous nerves, and not those of the mucous membranes, the group of respiratory motions is not brought into associated action, but there more usually occur spasms of the muscles of all the nerves of the trunk without spasmodic respiratory motions. The highest degree of this is the epileptic spasm from local nervous affections and the tetanus traumaticus from injury of a nerve.

"If the first demonstration of the phænomena of reflection in the first part of this manual * which appeared in the spring of 1833, and which I have here enlarged with reference to the observations of Van Deen, be compared with Dr. Marshall Hall's demonstration, a remarkable correspondence is found in the ideas and instances.

[To be continued.]

* See note, p. 53.

XXVIII. *Reply to* Dr. Ritchie's *Remarks. By the Rev.* J. W. MacGauley.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

YOU will oblige me by inserting in your next Number the following reply to Dr. Ritchie's "Remarks" on my paper, given in your Journal for last month.

I may agree with Dr. Ritchie, that there have been many writers and few readers on electricity and magnetism; but this seems to be a favourite position of his, if we may judge from the continual effort he makes, as is well known, to prove that almost every writer on the subject has neglected to read at least his experiments; I also admit that the person best able to come forward on the present occasion, has *wisely* left it to others; and I am greatly mistaken if maturer reflection shall not induce him to believe that he also had done *wisely* in imitating such an example. I did characterize his remarks at Bristol, on the paper which I then read, as *bitter* and *uncalled* for, because they were intended to deprive me of a claim to *originality*, and because, like the present, they were unfounded in fact, and irrelevant to the purpose. Should any of the expressions I use appear harsh or uncourteous, Dr. Ritchie will easily pardon me if he recall to his remembrance some of those he himself has not hesitated to adopt. Your readers are told that my first position,—"The spark and shock obtained from an electro-magnet, on breaking battery communication, are not the spark and shock of the battery, nor of the electro-magnet, but most probably the electricity induced on the wire of the helix by the electricity of the battery, or, if it be true that a current passes along the wire, the electricity intercepted in its passage from the copper to the zinc,"—is to be found in a paper of his contained in your Number for last June. Though I presume he has placed *me* in his catalogue of those who *write*, but do not *read*, I happen to be very familiar with the few documents he is able to quote. On looking again to the paper he mentions, what do I find? An account of what *he* deems an improvement of *his*—although many are found who deem the contrary—on the *magneto-electric machine*, and a theory which he builds on its results; but not one word as to whether the *spark* and *shock* of an *electro*-magnet are derived from the battery or magnet, or any other given source, which is just the thing, and the only thing, my first position contemplates. Besides, in his paper

he speaks of a *magneto-electric machine*, an apparatus long in use, and which requires a permanent magnet, but no battery; I, in my paper, of an *electro-galvanic* helix, a species of electrical machine which requires a battery but no permanent magnet: he, of an arrangement whose expense must be considerable, and whose power can never be great; I, of one easily obtained, and of almost unlimited energy. If he maintains that he *has* anticipated me in his *inquiries* on the subject, then they are most unfortunate, since their results are perfectly at variance with the truth. I shall quote his own words: "It is a well-known fact, that we receive a more powerful shock when electricity is being induced on a body, than when the induced electricity is returning to its natural state." Now it so happens that in the apparatus I describe in my paper, *no* shock is obtained when the electricity is *being induced*, a most powerful one when it is *returning* to its natural state. So much for Dr. Ritchie's claim to my first position.

He tells us my second position—"The spark and *shock* do not depend, except within certain limits, on the size of the battery,"—is found in Dr. Faraday's papers, "On the length of the coil influencing the *spark*." I presume he arrives at this conclusion by the same process of reasoning as that by which he inferred my first position to be one of his discoveries. But first, in treating of the spark, Dr. Faraday would not necessarily have included the shock, since they are known to be influenced by very different laws; and secondly, in these celebrated papers the inquiry is about the spark from the *secondary* current, obtained not from an *electro-* but a *permanent* magnet, and without the agency of a galvanic battery, which my second position supposes.

He says that in my third position I assert, that "magnetism within a helix proportionably injures its effect." He quotes, indeed, words which I have used, but he gives anything rather than the principle I affirm, simply because the context is suppressed. Why, if I take an isolated expression of his, I can make *his* talk very egregious nonsense. The assertion I do make is this, and I repeat it,—that if the iron of an electro-magnet retain, from the nature of its material, the presence of a keeper, or any other cause, the magnetism induced upon it, the shock and spark will be proportionably diminished, because the magnetism of the bar, by its inductive action on the helix, would prevent the perfect restoration to equilibrium of the electricity disturbed in the helix, by giving to the bar in a greater or less degree the nature of a permanent magnet, from which, by means of a helix coiled

around it, neither shock nor spark can be obtained,—will any one deny this except Dr. Ritchie? I believe not.

He tells us my fourth position—"The real power of the battery is not *increased*, but *diminished* by the electro-magnetic, or rather electro-galvanic helix,"—was known long since, and that Sir Humphry Davy was well acquainted with it. Sir Humphry Davy knew that the shorter the wire connecting the poles of an ordinary galvanic battery the better the effect, but that the great power *we* may obtain by means of the *long wire* of the helix and a battery is not the *increased power* of the *battery*, as others of high celebrity long after his time have supposed, is what he neither contemplated nor examined. My paper does not consider the simple wire connecting the poles, or an ordinary battery, but the peculiar and seemingly anomalous action of a peculiar arrangement of wire in *great length*, and possessing very peculiar properties.

Dr. Ritchie concludes by saying, "that the only thing *new* in my paper is a *fact* which is *not correct*, of which any one who possesses a magneto-electric machine may easily satisfy himself." In the name of common sense, what induces Dr. Ritchie to talk of a *magneto-electric* machine on this occasion! This mistake seems to have run through his mind the whole time he was criticizing and so severely condemning me, and what is worse, to have been uncorrected for many months: not a word about such a machine, nor a single experiment made with it, is found in my entire papers. I make experiments with one apparatus, he tries the same experiments with a very different and inappropriate apparatus; and then, forsooth, because his results are different from mine, he most kindly remarks that my only new fact is a false one. Is not this *bitter?*—is it not *uncalled for?* The circumstance he denies was noticed by several when I experimented in the Theatre of the Royal Dublin Society, and from whatever cause it may have arisen, was undoubtedly worthy of attention. I trust that when Dr. Ritchie next honours me with "remarks," if they are not ushered in by a more kindly prelude, they will at least be substantiated by facts, and sustained by more conclusive reasoning.

Your obedient Servant,

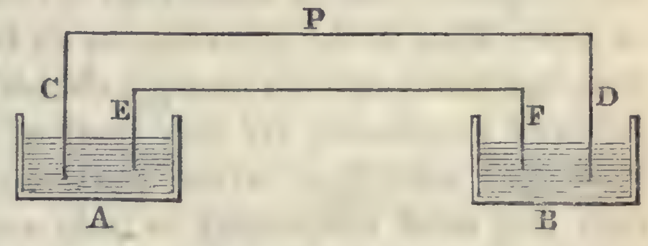
79, Marlbro'-street, Dublin, JAMES WILLIAM MACGAULEY.
January 9, 1837.

XXIX. *Further Experiments on a peculiar Voltaic Condition of Iron. By Professor* SCHOENBEIN *of Bâle; in a Letter to* Mr. Faraday.

DEAR SIR,

SOME weeks ago I had an opportunity to send you a paper "On a peculiar action of Iron upon some Salts," which I hope will by this time have reached you. Having since observed some new facts regarding the transference of the active and inactive state of iron from wire to wire, facts which I think to be of some importance to electro-chemical science, I take the liberty to communicate them to you by writing.

First Fact.—A and B represent vessels containing nitric acid, sp. gr. 1·35, and C P D a platina wire connecting them. If the oxidized end E, of an iron wire E F, be put into A, and F afterwards into B, F becomes active, though a current passes from F through the acid into D (the usual condition for calling forth the peculiar state).



Second Fact.—If C P D be a wire of a metal, which is acted upon by the acid in A and B,—for instance, silver, copper, iron, brass, &c.,—the end F of the iron wire will turn inactive on its being plunged into B, after the immersion of the oxidized end E in A. The same effect takes place if the middle part of the connecting wire P consists of platina, and the ends C and D of silver, copper, &c.

Third Fact.—If C P D be an iron wire, its end D inactive, C active, and the end E (not oxidized) first plunged into A, and F afterwards into B, F becomes inactive, that is to say, assumes the state of D. The inactive iron end D may be replaced by platina, and the active one C by any metal, which is acted upon by the acid in A, without causing a change of result by so doing.

Fourth Fact.—If everything be precisely as in the foregoing case, but E oxidized and first put into A, F becomes likewise inactive on its being afterwards immersed in B.

Fifth Fact.—If C P D be an iron wire, the end D inactive, (made so not by heating, but by immersing it in strong nitric acid,) and the end F put into B, and E afterwards into A, not only E, but also D turns active. Whatever may be the number of wires similar to C P D, all their inactive ends in B turn active under the circumstances mentioned, though these wires do not touch each other at any point.

Sixth Fact.—If the four electrodes of two piles (each consisting of about half a dozen of pairs of zinc and copper) be introduced into two vessels containing common nitric acid, in such a manner that the positive electrode of one pile and the negative one of the other dip into the same vessel, and the oxidized end of an iron wire be plunged into any of the vessels, and its ordinary end afterwards into the other one, the latter becomes inactive, just in the same way as if the two vessels were connected by a copper wire. But to obtain this result it is requisite to bend up the second, that is to say, the ordinary end, thus  previously to immersion.

Now, why does F in the first case not become inactive by the current produced by its being plunged into B? It seems to be an indispensable condition for calling forth the inactive state in iron, that at the moment of its being immersed into the acid, a current of a certain energy should be passing through it. The current produced by the part of the metal immersed, is of sufficient strength when both ends of the iron wire plunge into the acid contained in only one (small) vessel; but when this same current has to pass through the acid of two vessels, and besides to enter and issue into and from the connecting platina wire, its strength is diminished below the degree necessary for producing the effect in question. But if this way of accounting for the fact be correct, it may be asked, how it comes, that with a connecting wire whose ends are attacked by the acid of the vessels, different results are obtained? It is obvious, that in the second case, two currents moving in opposite directions and originating in C and D are established, as soon as the iron wire E F has connected the vessels A and B. Besides these currents, a third one is produced by the immersion of F in B. But this current having to move the same way which the current in the first case must pass, why is its effect different from what that of the latter is? Now it seems to me, that if two currents of opposite directions circulate through our circuit of the second case, they remove in some way or other the obstacles which the third current (in itself of weak power) would have to overcome, if it were moving alone through the circuit; or in other words, if two opposite currents cross the nitric acid, its conducting power for a third current is increased. In the third case, there are likewise two opposite currents established, as soon as F dips into B; one produced by C, the other by E; and there is again a current excited by F, which must be considered as the cause of the peculiar state of this end. It is only to be wondered at why D, when having been made inactive by immersion in strong nitric acid, or by the help of platina, is not rendered

active by the current produced by F; for, the same reasons why F turns inactive, should throw D into action. But from many facts it appears, that a much stronger current is required to change the inactive state into the active one, than that required to render an ordinary wire inactive. The fourth fact will be accounted for, if we consider that in this case a current passes from C to F, which added to that produced by F itself, becomes strong enough to call forth the inactive state in F, though it is still too weak to render D active; and probably only so on account of the absence of two other opposite currents. As to the fifth case, E becomes active, because in the moment of its immersion there are no two opposite currents put into circulation; the current produced by E is therefore too weak to excite in E the peculiar state; and there are besides the two currents of C and F, which would more than neutralize the current of E. Now the current originated by F being continuous, and besides powerful, compared to that excited by an iron wire becoming inactive, would of itself throw D into action, but its energy is still increased by the two opposite currents produced at C and E. About the sixth fact I say nothing, as its connexion with the foregoing ones is sufficiently clear.

I allow that the inference I have drawn from the facts stated is rather hazardous, and in apparent contradiction to the generally established principle, that two equal but opposite currents annihilate each other, and that the circuit through which such currents move, is exactly in the same state as if no currents were passing through it. But I think that without adopting my view of the subject, the facts spoken of remain quite unaccountable. Whatever cause, however, they depend upon, in my opinion they deserve to be closely searched into, as their minute investigation will, no doubt, lead to interesting results.

The last Number of the *Bibliothèque Universelle* contained a paper of mine on the relation of iron to oxygen, which happens to be full of most unhappy misprints. They will, no doubt, be corrected in the forthcoming Number. I am very anxious to know your opinion about the contents of the said paper.

Begging your pardon for having repeatedly intruded upon you a badly written letter, I take the liberty of calling myself,

Yours very truly,

Bâle, Dec. 26, 1836. SCHOENBEIN.

XXX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

Nov. 16, 1836.—A Paper was first read "On indications of changes in the relative level of Sea and Land in the West of Scotland," by James Smith, Esq., of Jordan Hill, F.G.S.

In the West of Scotland are two superficial deposits. The lowest, in some districts called "Till," consists of stiff unstratified clay, confusedly mixed with boulders. It rarely contains organic remains, but stags' horns, tusks, and bones of the elephant have been found in it in the bed of the Union Canal at Kilmarnock, and remains of the elephant associated with marine shells at Kilmaurs in Ayrshire.

The upper deposit is composed of finely laminated clay, overlaid by sand and gravel; and marine remains of existing species occur in every part of it, but most abundantly in the clay. It has been traced by Mr. Smith, on both sides of the Clyde from Glasgow to Roseneath and Greenock, at points varying from 30 to 40 feet above the level of the sea. He has also observed sea-worn terraces on each side of the Clyde below Dumbarton and between Cloch Light-house and Largs.

The following are the principal localities, mentioned in the paper, at which the clay bed has been examined.

A brickyard at Glasgow, 30 feet above high-water mark, where the author found the remains of six species of recent marine shells of common occurrence on the adjacent coasts of Scotland; also a branch of an elm and an oak-tree with its roots. The canal from Glasgow to Paisley and Johnstown was excavated in the clay at the height of 40 feet above the sea, and numerous remains of 26 species of existing marine testacea were found in it. In a pond lately dug at Paisley, a bed of clay was exposed, to which a violet colour had been given by decomposed muscles, in a manner similar to that described by Mr. Lyell in his memoir on change of level on the coast of Sweden*. In the brick and tile works around Paisley, and in the adjoining parishes, recent shells are abundant. Near Renfrew, cockles are so numerous, that a farm and hill, are called Cockle Farm and Cockle Hill. At Johnstown, which is about 8 miles from the sea and at a point about 40 feet above its level, in making a well, there were found bones of fishes and sea-fowls, fragments of sea-weeds, crabs' claws, and numerous layers of shells imbedded in sand and clay, which rested on a deposit of "till" more than 70 feet thick. Besides these localities, recent shells have been noticed at Helensburgh, also near Loch Lomond, at Dalmuir, and the shores of the Firth of Forth.

With respect to the origin of these deposits, Mr. Smith is of opinion, that the lower or "till" resulted from the violent though transitory action of a body of water; but that the upper was gradually deposited at the bottom of a sea of sufficient depth to protect it from the agitation of waves, and that it was raised to its present level by a process

* Phil. Trans., 1835, pp. 5, 7. [An abstract of Mr. Lyell's memoir appeared in Lond. and Edinb. Phil. Mag., vol. vi. p. 297.]

analogous to that described by Mr. Lyell as now taking place on the shores of the Baltic*.

Of the period when the change was effected, Mr. Smith offers no conjecture; but he states that it must have been anterior to the occupation of Britain by the Romans, because the terminations of their wall on the shores of the Forth and the Clyde were constructed with reference to the present level of the sea. He also adds, that on the banks of the Firth of Clyde there are vitrified forts and tumuli to which the same observation applies; and that no human remains or works of art have been found in the clay.

At his first examination, the author concluded, judging from the sea-worn terraces which skirt the coasts, that the change of level could not exceed 40 feet, but he has since observed the clay at the height of 50 feet; and Mr. Buchanan of Arden has found oyster-shells near Loch Lomond 70 feet above the sea. Mr. Smith, however, believes that at the period when the clay was accumulated and the terraces formed, the relative level of sea and land was stationary, and that, if we may judge from the comparative dimensions of the ancient terraces with those now forming, the period during which the level was thus stationary must have greatly exceeded 2000 years.

The important question, if the Fauna and Flora of the period when this deposit was accumulated were identical with those of the present epoch, Mr. Smith says it would be premature now to determine. A very great proportion of the species of shells, amounting in all to about 70, abound in the present seas; and it is worthy of remark that *Astarte Garensis*, which is common in the clay at Helensburgh, is found in great numbers in the Gare Loch; on the other hand, some of the species have become very rare, if not extinct with reference to the coast of Scotland.

In alluding to the geological position of the upper deposit, the author says, that it must be placed among the newer pliocene; and as it belongs to one of the first steps in the descending series, every circumstance connected with it should be carefully observed and recorded, that researches into the more ancient formations may be conducted with greater success.

A paper was afterwards read "On the distribution of Organic Remains in the Oolitic formations on the coast of Yorkshire," by W. C. Williamson, Esq., Curator of the Natural History Society of Manchester, and communicated by the President.

In a former paper† Mr. Williamson gave detailed sections of the lias in the Yorkshire coast, with a view to determine how far its fossils might be useful in recognising the different beds of the formation at other localities. The paper read at this meeting was prepared with the same intent, and gave detailed accounts of the fossils of the (1) inferior oolite, (2) the lower shale and sandstone, and the (3) Great or Bath oolite.

1. The point selected by Mr. Williamson as affording the best section

* Phil. Trans., 1835, p. 1.

† Lond. and Edinb. Phil. Mag., vol. v. p. 222.

of the inferior oolite on the Yorkshire coast is Blue Wick; and the following is the succession of the strata which it presents in ascending order:

		Feet.
1.	Thick beds of dark grey finely grained sandstone ..	20
2.	Irregular beds of yellow sandstone..............	20
3.	Hard ironshot sandstone containing small pebbles..	$1\frac{1}{2}$
4.	Irregular beds of yellow sandstone, in some parts ironshot, and inclosing layers of pebbles........	30
5.	Hard ironstone, many fossils	4
6.	Hard ironstone, no fossils	8

The beds No. 1. contain, in their lower part, argillaceous nodules resembling those of the Alum shale, and inclose great numbers of *Ammonites striatulus*, *Lingula Beanii*, *Orbicula reflexa*, and, in less abundance, an Avicula, resembling *A. echinata*, and *Terebratula bidens*, Above this nodular bed no fossils have been noticed till within 10 feet of the top of No. 1, where another layer of similar concretions occurs, inclosing portions of an Astacus resembling in some respects *Astacus rostratus*, and a species agreeing with one found in the Cornbrash. A little nearer the top of No. 1. is a thin seam containing great numbers of *Vermetus compressus*, which is found also in the coralline oolite of Yorkshire.

The division No. 2. presents throughout its whole thickness small fragments of dicotyledonous wood with an undescribed Belemnite; and towards the top are found, though rarely, *Mya litterata*, and still higher two species of Ammonites apparently new.

The bed No. 3. contains the same Belemnite as No. 2, but is characterized by great abundance of *Terebratula trilineata*.

No. 4. is destitute of fossils, except near its junction with the overlying bed, where it contains the Belemnite and Avicula of Nos. 1. and 2.

The bed No. 5, though not more than 2 feet thick, incloses the greater part of the fossils of the inferior oolite of the Yorkshire coast, and the following is a list of the species given by Mr. Williamson, as occurring chiefly in the middle and lower portions of the bed:

Trochus granulatus, T. bisertus, T. pyramidalis, Solarium calix, Nerita costata, Turbo lævigatus, T. funiculatus, Rostellaria composita, Natica adducta, N. tumidula, Terebra vetusta, Acteon humeralis, Auricula Sedgvici, Ostrea Marshii, O. solitaria, Pecten virguliferus, P. abjectus, Trigonia angulata, T. costata, T. striata, T. gibbosa, Avicula Braamburiensis, Astarte elegans, A. minima, Modiola pulchra, M. cuneata, Mytilus sublævis, Cardita similis, Nucula axiniformis, Isocardia concentrica, Cardium incertum, Pholadomya ovalis, Unio abductus, Gastrochæna tortuosa.

The upper part of the stratum is characterized by *Turritella muricata*, *T. quadrivittata*, *T. humifusa*, *T. cingenda* (Phillips) in great abundance, *Melania Heddingtonensis*, *Terebratula obsoleta*, and *Caryophyllia convexa*.

In No. 6, the highest stratum of the inferior oolite, no fossils have been noticed by Mr. Williamson.

2. Immediately above the inferior oolite lies the lower sandstone and

shale, an important formation on account of the absence of marine remains and the presence of terrestrial plants. With the exception of a similar series of beds above the great oolite, this formation is the most irregular in its subdivisions of any on the Yorkshire coast. The only point at which the upper and middle divisions of the series are fully developed are the cliffs between Cloughton Wyke and Blue Wick; the remainder of the coast exhibiting only the lower divisions. The following is the succession of the beds in ascending:

	Feet.
1. Black carbonaceous shale, no vegetable remains ..	10
2. Hard, pale, gritty sandstone, containing at its junction with No. 1. great abundance of a new species of Calamites, also fronds of *Zamia gigas*, and a remarkable fossil apparently connected with the fructification of a Cycas....................	20
3. Shale	10
4. Gritty sandstone	20
5. Softish sandstone containing fine specimens of *Equisetum columnare*, all in a vertical position with their roots downwards	8
6. Soft black shale	3
7. Sandstones and shales..........................	170
8. Dark shaly sandstone	8
9. Hard grey sandstone	6
10. Black shale	2
11. Laminated sandstone containing great abundance of various species of beautiful ferns, Cycadean plants, and Equiseti, also towards the lower part three seams of soft jet	6

The following are the species enumerated by Mr. Williamson:—*Equisetum laterale, Lycopodites Williamsonis, L. falcatus, Thuites expansus, Sphenopteris longifolia, S. hymenophylloides, Pecopteris ligata, P. curtata, P. Whitbiensis, P. Williamsonis, Pterophyllum pectinoides, P. minus, Otopteris acuminata, Cyclopteris digitata, Tæniopteris vittata, Solenites Murrayana.*

	Feet.
12. Sandstones and shales containing no well-preserved plants, but about 90 feet from the top a bed of coal 1 foot thick	170

The plants of this system differ from those of the upper sandstone and shale by the abundance of *Pterophyllum minus, Otopteris acuminata, Sphenopteris hymenophylloides,* and the deeply lobed *Cyclopteris digitata,* and is characterized by *Pterophyllum pectinoides, Equisetum laterale, Lycopodytes falcatus,* and by a singular frond supposed to belong to a Cycadean plant. With respect to the vertical Equiseti in bed 5, Mr. Williamson is of opinion that they did not grow where they are found, but were transported, not, perhaps, from a great distance; and that their perpendicular position is owing to the roots of this description of plants being specifically heavier than the stem.

3. The Great or Bath oolite varies very little in its characters or fossils, except for 8 or 9 feet from the top, where, according to Mr.

Williamson's observations, it presents two forms very different both in structure and organic remains. The localities referred to in the memoir for the general structure of the formation are Cloughton and White Nab, and for the upper beds Cayton and Gristhorpe Bays.

Section of the general structure of the Great Oolite.

	Feet.
1. The lowest beds consist of a very hard blue limestone, sometimes oolitic and destitute of fossils, except in the upper part, where the stone is softer, and where the author has found *Ostrea edulina*, *Amphidesma decurtatum*, *Mya calceiformis*, a large undescribed Ammonites, and at the junction with the next bed *Belemnites compressus*, *B. Aalensis*, *Melania Heddingtonensis*, *Amphidesma decurtatum*, Serpulæ, and long tuberculated spines of a Cidaris, but no portion of the Cidaris itself....	14 to 20
2. Hard blue fine-grained oolite, sometimes ironshot, and apparently devoid of organic remains	6
3. Soft or hard bluish clay, tinged, at some localities, with iron. It contains at least 11 species of fossil shells, which are most abundant at Cloughton Wyke, and some species which occur there have not been noticed at any other point. This bed also contains the remains of a Saurian, which the author is induced to consider a new species of Plesiosaurus..	2

The shells given by Mr. Williamson are, *Rostellaria composita*, *Acteon glaber*, *Terebra vetusta*, *Phasianella cincta*, *Trochus*, *Avicula Braamburiensis*, *Gervillia acuta*, *Cucullea cancellata*, *Astarte minima*, *Cardita similis*, *Pholadomya acuticostata*.

4. Nodular ironstone, sometimes inclosing fragments of *Ammonites Blagdeni*	6 to 12 in.
5. Clay containing *Avicula Braamburiensis*, *Amphidesma decurtatum*, and a *Gervilia*	1 ft.
6. The top strata consisting of layers of nodular ironstone and argillaceous oolite	3

The lower part of this bed is characterized by the presence of *Perna quadrata*, and the upper by numerous remains of the following shells:

Melania Heddingtonensis, *Ammonites Blagdeni*, *Terebratula spinosa*, *Gryphæa nana*, *Ostrea Marshii*, *Pecten lens*, *Plagiostoma interstinctum* (c)*, *Avicula Braamburiensis* (c), *A. echinata* (c) ? *Gervilia acuta* (c), *Trigonia costata*, *T. clavellata*, *Astarte minima* (c), *Corbula depressa*, *Pinna cuneata*, *Pentacrinites vulgaris*, *Cidaris vagans*.

The author then describes the upper beds of the Great Oolite at the two extremities of Cayton Bay, and at low water at the south side of Carnelian Bay.

Top. Irony nodules, without organic remains........	1 ft.
Extremely hard ironshot rock, composed almost wholly of fragments of fossils, viz. *Millepora straminea*, papillæ of a Cidaris, innumerable small spines, probably	

* (c) The species thus distinguished are found in the greatest abundance.

of *Cidaris vagans*, muricated spines, and joints of a Pentacrinites . . . . 8 ft.

This bed Mr. Williamson appears to consider peculiar to the localities mentioned, not having observed it elsewhere.

He afterwards describes the upper beds of the Great Oolite at the south point of Cayton Bay. Immediately above the nodular ironstone bed is a very thick series of sandstones and shales surmounted by a seam of argillaceous oolite, containing *Avicula Braamburiensis*, and similar to that which forms the top of the Great oolite at White Nab (No. 6. of the section), with the exception that the nodular ironstone is wanting. This system of sandstone and shale, considered by Mr. Phillips to belong to the upper marl and sandstone, is, in Mr. Williamson's opinion, a distinct and merely local deposit included in the superior division of the Great oolite. It contains most of the fossil plants assigned by Mr. Phillips to the upper marl and sandstone, and several which are new and peculiar to it. An ascending sectional list is given of this deposit, commencing with the 1 foot bed of irony nodules, and passing through 35 feet of alternations of shale, with ferruginous and other sandstones, ends in the seam of argillaceous oolite, which is said to be overlaid by the upper sandstones and shales. One of the beds of shale contains a vast number of plants, amounting to above 40 species. The following list is given by Mr. Williamson:

Pecopteris lobifolia, P. insignis, P. undans, P. polypodioides, P. propinqua, P. Williamsonis, P. acutifolia, P. obtusifolia, P. dentata, P. exilis, P. cæspitosa, Neuropteris recentior, N. ligata, N. arguta, Sphenopteris stipata, S. Williamsonis, Cyclopteris digitata, Glossopteris Phillipsii, Tæniopteris vittata, T. major, Solenites Murrayana, Lycopodites Williamsonis, Sphæridia paradoxa, Pterophyllum comptum, P. Pecten, P. minus, Otopteris cuneata, O. Beanii, Ctenis falcata, Dictyophyllum rugosum, Cycadites tenuicaulis.

Two shells are occasionally found in these beds, and are considered by the author to be allied to the genus *Anodon*.

In conclusion, Mr. Williamson says, that the characteristic shells of the Great oolite are few, as they bear a general resemblance to those of the cornbrash and inferior oolite.

ROYAL SOCIETY.

Address of His Royal Highness the President, delivered at the Anniversary Meeting, Nov. 30, 1836.

GENTLEMEN,

I APPEAR before you, after an absence of two years from this chair, under circumstances which deeply affect my feelings. I have been secluded, during nearly the whole of that period, from the active business of life and of society, by the slow but sure approaches of almost total blindness; by preparations for a most delicate and, to me, most important operation, and by the precautions which were necessary to accomplish my recovery, after it had been most skilfully and successfully performed. In resuming now, therefore, my public duties in this place, I feel sensibly the novelty of my situation, as if I were entering, by the blessing of God, upon a new tenure of existence, which, whilst it offers to my view many

prospects of happiness, imposes upon me likewise heavy responsibilities; and I can only express my fervent hope and prayer, that the same merciful Providence which has vouchsafed, through his appointed means, to restore me to sight, may enable me, like a willing and humble-minded scholar, to apply the lessons taught me by the experience of my past life, to the just and useful regulation of that portion of my course which I may be still permitted to run.

It is my first and most pleasing duty, Gentlemen, to thank you for your congratulations upon my recovery, which have been conveyed to me in terms most grateful to my feelings. I have on many occasions experienced both your kindness and forbearance, and I deeply regret that circumstances should so frequently have compelled me to appeal to them: but at no moment could the expression of your good-will be more welcome to me than at the present, when I am enabled to reappear amongst you, upon being again entrusted with the possession of that blessing, the value of which I have learnt to appreciate more fully by my experience of its privation.

Could I have foreseen, when the progress of my malady first removed me from public life, the length of time which was to elapse before its termination, even in case I could have felt assured that it would end as fortunately as it has for me, I would not have ventured to trespass, so long as I have done, upon your indulgence, but would at once have retired from the proud situation of your President; for though I could rely with perfect confidence upon the cordial cooperation of the members of the Council, and should have felt satisfied that they would not allow the real interests of the Society to suffer from my absence, yet I could not have continued altogether free from alarm, lest its dignity should be lowered in public estimation, were its affairs long allowed to be conducted with an incomplete establishment; or the becoming authority of this Chair should be lessened by frequent changes in its occupation, particularly on great and public occasions. I was always led to believe that the disease under which I laboured would have been sufficiently advanced to justify an operation much sooner than eventually proved to be the case, and I was therefore induced to hope that my absence from the Society would not have been prolonged for such a period as to be productive either of reasonable complaint, or of serious inconvenience. When, however, the day of your last Anniversary approached, and that hope had proved delusive, I felt it my duty to resign my trust, however reluctant to sever myself from a body with which I am so honourably connected; and I only consented to continue in its occupation, when kindly pressed to do so by the members of the Council, under the conviction that the time for performing the operation was so near at hand, that its success or failure would speedily decide whether I should be capable of again taking an active part in our concerns, or be compelled to terminate my official connexion with you for ever. I thank God that I am now enabled, in person to express my heartfelt gratitude for your kindness to me on all occasions, and especially on the present; and I beg you to feel assured

that the remembrance of your sympathy with my affliction whilst it continued, and of your warm congratulations upon my happy recovery, will ever tend to cement more closely the ties of affection and friendship which subsist between me and the Fellows of the Royal Society.

My necessary absence from my duties amongst you will prevent my entering in much detail upon the ordinary transactions of the Council, and of our weekly meetings during the last year; for a particular statement of which I must refer to the Report of the Council, which will be read to you by one of your Secretaries, Dr. Roget. There are only two topics connected with them to which I feel myself particularly called upon to allude.

The first is the publication of the classed catalogue of our library; the second relates to the discussions which have been attempted to be raised upon the Minutes of your proceedings on the ordinary days of your assembling during the last year.

It is well known to you, Gentlemen, that, after the transfer of the Arundelian MSS. to the British Museum, and the great additions which your library received from purchases and exchanges of books, necessarily consequent upon that transaction, Mr. Panizzi was employed by the Council to draw up a classed catalogue of its contents. Such a compilation it was considered would be of great value, not merely to the Fellows of the Society but to men of science generally, by making known to them the treasures of a library singularly rich and complete in journals, and works on mathematical, physical, astronomical, and anatomical science, and by presenting them in such a form that persons engaged in works of research, or in any specific subject of scientific inquiry, might be made at once acquainted with nearly all the sources from whence they could derive information. This catalogue is now printed, or more correctly speaking, *composed*, and is undergoing such a revision from different Members of the Council, who have kindly undertaken this task, as is calculated to make it as correct and complete as the circumstances of the case will allow it to be. I have reason to hope that this work will be shortly placed in the hands of the Fellows, and that the example which it will present of what may be accomplished by the exertions of a learned body with very limited funds at its command, will not be without its influence in hastening the completion of a similar work with respect to our great national library, upon a scale proportionate to its importance, and worthy of a great and wealthy people, amongst whom literature, science, and the arts are duly cultivated and pursued.

The discussions that have at different times during the last year been raised upon the Minutes of your proceedings, constitute the second subject which I wish especially to notice.

I am quite sure, Gentlemen, that you will agree with me in thinking, that no one circumstance has contributed so effectually to maintain the dignity of the Royal Society, as the prohibition of personal debate in the transaction of its ordinary business; and if I wished for any additional confirmation of this opinion, I would

appeal to the very serious amount of irritation which it produced amongst you in the course of the last year, though originating in the most trivial causes. It was chiefly with a view to avoid inconveniences of this kind, and to provide an outlet for the proper expression of opinion, when any just occasion of complaint might exist, or any extraordinary circumstance occur, and to terminate disputes whenever unfortunately they might arise, that the Council, at the last revision of our statutes, passed a by-law, as they were fully authorized to do, which makes it imperative upon the President and Council to call an extraordinary meeting of its Members, upon the due presentation of a requisition for that purpose, signed by at least six Fellows, and setting forth, in specific terms, the objects for which it was required to be summoned, provided those objects be not inconsistent with the charter and statutes of the Society. Such extraordinary meetings being strictly domestic, and confined to the Fellows of the Society only, appear to me not merely to offer a sufficient security against any great mismanagement of the affairs of the establishment, but likewise to protect your ordinary meetings from those irregular and somewhat tumultuary discussions on matters of business, or personal conduct, which might otherwise be in danger of arising.

I believe that many persons have expressed a wish that the regulations of this Society should be so far relaxed as to allow, in conformity with the practice of some other similar establishments, discussions upon the papers, and those papers only, which are read before us: I confess, for my own part, that I am not at present prepared to accede to this recommendation. A practice which has been sanctioned by the usage of more than a century and a half, and found to be productive of scientific results unrivalled for their extent and value, should not be abandoned by us without the most mature consideration; and though I am the last person to recommend a slavish submission to the dictates or to the customs of antiquity, which may be unsuited either to the altered circumstances of modern times, or incapable of defence upon other and independent grounds, yet a reverence is justly due both to maxims and observances which have been sanctioned by high authorities, or connected with great and important public benefits. It may be quite true that such discussions would tend materially to increase the personal interest which is taken, by many of our members, in our proceedings; but when we consider the abstract and abstruse nature of many of the papers which come before us, and which no single reading can make perfectly intelligible, even to the best-instructed hearer, as well as the vast variety of subjects which they comprehend, I think we may fairly infer that such discussions would rarely add much to the stock of facts or of reasonings which they contain, or that their influence would be materially felt in the publications of your Transactions, which have always formed, and which ought always to form, the great object of the foundation of this Society, and the only means by which its character and influence can continue to be maintained unimpaired throughout the civilized world. When we likewise take into further

consideration the irregularities and personalities to which such debates would on some occasions give rise, unless very strictly limited and very authoritatively controlled, as well as the indirect influence which the premature expression of opinions upon the contents and merits of individual papers might exercise upon the decision of the Council in selecting them for publication, you will be disposed to agree with me, I trust, in thinking that such an experiment would be at least dangerous to the peace, as it very possibly might prove ultimately injurious to the scientific character, of the Royal Society.

But let me not be misunderstood: the success that has attended this practice in the institution which has contributed so powerfully to the rapid advance of a highly popular science, might appear to offer a practical refutation of such grounds of alarm as those which I have ventured to suggest; but the cases of the two Societies are extremely different. The science of geology is eminently a science of observation, where facts, collected from all quarters of the globe, and accurately recorded, possess a value which is in many cases independent of the theoretical inferences that may be deduced from them: it is a science which disdains not the aid of the humblest labourers who can widen the range of its observations; it is a science also in which both facts and theories can be communicated more accurately and more rapidly by a graphic and vivid oral description, aided by an immediate reference to maps, drawings and specimens, than by the most elaborate and laborious written descriptions; it is a science which can only be learnt by being seen, and which can only be seen through ten thousand eyes. In all these, and in many other important particulars, it differs from the majority of those sciences which most commonly come under the notice of the Royal Society; and the many circumstances which not only justify, but in some degree render necessary, the discussions upon the papers read, or the facts communicated to the Geological Society, would almost entirely cease to apply if extended to us. And when we further consider the varied knowledge and accomplishments, the lively wit and rare eloquence of many of those distinguished men who usually take part in those debates, and who are themselves the highest authorities in the very science which on such occasions they are called upon to illustrate and to teach, we should be disposed rather to regard them as lectures delivered by great masters to pupils who come to learn, than as the discourses of philosophers, amongst each other, upon the more abstract and less attractive departments of human knowledge.

And now, Gentlemen, before I conclude this portion of my address, there remains but one other point which I think it my duty to notice. A trust of great importance, imposed on the President of the Royal Society by the will of the last Earl of Bridgewater, the most onerous and responsible duties of which devolved upon my worthy friend and predecessor Mr. Davies Gilbert, is at length terminated, by the appearance, which has been long and anxiously expected, of the eighth Treatise of the series. It would ill become me to speak of the mode

in which that important duty was discharged by him, or of the principles which guided himself and his distinguished assessors, in the selection either of subjects or of the authors; but a list which is headed by the name of Whewell and closed by that of Buckland, can hardly be considered as an unworthy representation of the science and literature of this country.

Amongst the losses sustained by the Society during the last year, will be found many names of persons distinguished for their services both in literature and in science; and if we might be allowed to form a judgement from the very great proportion of these eminent men whose ages have approached the extreme limits of human life, we might conclude with great confidence that the most severe studies and the most trying climates, if pursued with temperance or guarded against with care, are not unfavourable either to health or longevity. The list which has been placed in my hands contains the names of twenty-one Fellows and two Foreign Members, and I greatly regret that the notice which I am enabled to take of some of the most distinguished of their number should be necessarily so slight and imperfect.

Mr. Pond succeeded Dr. Maskelyne as Astronomer Royal in 1810, and retired from that important situation, under the pressure of many infirmities, in the autumn of last year: he was formerly a member of Trinity College, Cambridge, where he was a pupil of Professor Lax, whose name appears also in the list of deaths which has been just read to you. After leaving the University, he travelled in many parts of the East, and particularly in Egypt, partly urged by the spirit of adventure which is natural to youth and partly with a view of making astronomical observations in climates more pure and more regular than our own. After his return home in 1800, he settled at Westbury, in Somersetshire, and devoted himself, amidst other pursuits, chiefly to astronomy, making use of a circular instrument of $2\frac{1}{2}$ feet diameter, which had been constructed and divided by Troughton with more than ordinary care. With this instrument he observed by a peculiar method, the declinations of some of the principal fixed stars, which were communicated to the Royal Society in 1806; and it afterwards enabled him to establish the fact of a change of form in the great quadrants at Greenwich, a discovery of great importance, inasmuch as it not only led to the substitution of circular instruments for them in our national observatory, but subsequently likewise to his own appointment as Astronomer Royal.

After Mr. Pond's establishment at Greenwich, he communicated to the Royal Society from time to time, not merely the general results of his labours, but likewise his views of the theory of astronomical observations and of the grounds of judging of their relative accuracy: his system was to observe differences of declination and right ascension, making every star a point of departure for the rest, and considering the pole as a point in the heavens whose position was capable of a determination, equally, and not more accurate than that of any given star. To such a view of the theory of observation, circular instruments were particularly adapted, and there is no reason

to doubt that the relative catalogues of the stars which were formed by Mr. Pond were more accurate and complete than those of any preceding or cotemporary observer. Such a result, however, might have been reasonably expected from the great powers and resources of the establishment over which he presided and which he had himself been the chief means of calling into action.

The method which was adopted by Mr. Pond to determine the limits of the annual parallax of certain fixed stars by means of fixed telescopes of great focal length, was singularly ingenious and complete. The existence and amount of such a parallax had been asserted and assigned by Dr. Brinkley, in α Lyræ, α Aquilæ, and α Cygni; but this opinion, although most ingeniously and even obstinately vindicated and maintained by him, was, in the judgement of most other astronomers, most decisively negatived by Mr. Pond, who showed that the parallax of those fixed stars, supposing its amount to be sensible, was confined within the limits of the errors of the most delicate and perfect observations which have been hitherto made. There is no great question in astronomy, the present position and limits of which are more satisfactorily settled.

Mr. Pond was remarkable for his skill and delicacy in the manipulation of his instruments, and no man was more capable of forming a correct judgement of their capacities and powers, and of the nature and extent of the errors to which they were liable: he was in the habit of placing great reliance on the results of a great number of observations, when no apparent or assignable cause existed for giving a determinate sign or character to the errors of individual observations: this confidence, however, was founded on his great knowledge of the theory of observation, and was fully justified by a comparison both of his own results with each other, and with those of other observers.

Mr. Pond was a man of gentle and amiable character, and singularly candid and unprejudiced. His health for many years before his death was greatly deranged, but he continued to struggle against the progress of his infirmities, and, from a conscientious feeling, he never abandoned the active duties of superintending the observatory, though hardly able to sustain them. He died in August last, at Lee, in Kent, and was buried in the tomb of his great predecessor Halley *.

Mr. Pond, though a great practical astronomer and a man of uncommonly clear intellect and correct judgement, was deficient in one very considerable qualification for the station which he filled,—I mean,

[* Various papers by Mr. Pond or relating to his observations and views in Astronomy have appeared in the Philosophical Magazine. His paper *On Changes in the Declination of certain Fixed Stars*, was reprinted from the Phil. Trans. for 1823, in Phil. Mag., First Series, vol. lxii. p. 175; the subject is noticed also at pp. 391, 453, 454, and 466, of the same volume, in which likewise will be found, at p. 292, Mr. P.'s memoir *On the Parallax of α Lyræ*. In vol. lxvi. p. 33, appeared a translation of a paper by M. Bessel respecting the former subject; and a discussion relative to the accuracy of the Greenwich Observations will be found in vol. lxiv. p. 367, 451, and vol. lxvi. p. 292.—Edit.]

an acquaintance with the higher branches of Analysis, and their application to Physical Astronomy. His successor, Gentlemen, is well known to you, and needs no eulogium of mine; but I cannot omit the opportunity which is now offered to me of congratulating the friends of astronomy and of science on the appointment of a gentleman to this most important office, who is second to none in this country in his great attainments in almost every department of accurate science, in his indefatigable and systematic industry, in his high sense of public duty, and in his profound knowledge both of physical and of practical astronomy.

The names which I shall next bring before your notice are those of three men, venerable alike for their great age and public services, and who must always be regarded as entitled to hold a distinguished place amongst that illustrious body of great men, who have been produced or brought forward by the important trusts, the varied employments, and, let me add likewise, the great rewards of our Indian empire; I mean Sir Charles Wilkins, Mr. Marsden, and Captain Horsburgh.

Sir Charles Wilkins went to India in 1770, and was the first Englishman who thoroughly mastered the difficulties of the Sanscrit language, of the classical works in which he published several translations, and smoothed the obstacles to its attainment by a noble grammar, which he composed for the especial benefit of the students of the East India college at Hayleybury, of which he was the oriental visitor and examiner from the period of its first establishment. He formed with his own hand the matrices of the first Bengali and Persian types which were used in Bengal, and he was the chief agent, in conjunction with Sir William Jones, in the establishment of the Asiatic Society of Calcutta, whose labours have contributed so greatly to the advancement of our knowledge of the languages and general condition of the provinces of our Eastern empire. It is now more than fifty years since he returned to this country, in possession of a competent fortune and vigorous health, which he continued to enjoy, in conjunction with every social and domestic comfort, with hardly any interruption, to the day of his death. Sir Charles Wilkins was appointed, in 1800, Librarian of the great collection of Oriental MSS., which are preserved in the India House; and this Society is indebted to him for the catalogue and description of the Sanscrit and other Oriental MSS., which were presented to it by Sir William and Lady Jones.

Sir Charles Wilkins was the father-in-law of Mr. Marsden, though nearly his cotemporary in age. They went to the East about the same time, and whilst one devoted himself to the study of the languages and literature of the ancient and modern inhabitants of continental India, the other availed himself of his position on the great island of Sumatra and the Malayan peninsula, to gain a thorough acquaintance with the present condition and past history of that active and adventurous race, whose character has been so deeply and so generally impressed upon the languages and customs of nearly all the tribes who inhabit the innumerable islands of the Indian Archipelago and of the Pacific Ocean. His account of Sumatra, which appeared soon after his

return from the East, may be considered as a model for all monographs of the history, languages, customs, and statistics of a particular nation. He subsequently published a Malay dictionary of great authority and value; and in many separate memoirs, one of which appeared the year before his death, he traced with great learning and research the general characters and analogies of the East Insular and Polynesian languages, and proposed an alphabet for their uniform and intelligible transcription. Mr. Marsden was the author of four papers in our Transactions on some remarkable natural phænomena in the island of Sumatra, on the Mahometan æra of the Hejira, and on the chronological periods of the Hindoos; the two last of which show a very extensive acquaintance with Arabian and Hindoo literature. He published very elaborate catalogues of his fine collections of vocabularies and grammars, and also of his oriental coins; the first of which he presented in his life-time to King's College, London, and the second to the British Museum. Mr. Marsden returned to England from the East at an early age, and was Secretary to the Admiralty during the most eventful period of the late war. He continued to enjoy to an extreme old age, extraordinary vigour both of mind and body, equally respected and beloved for his great learning and very varied acquirements, for his independent and disinterested character, and for his many social and domestic virtues.

Captain James Horsburgh entered the sea service of the East India Company at a very early age, and in a very humble capacity, and raised himself by his perseverance, good conduct, and strong natural talents to the command of a ship, in which he was employed, for a considerable time, in a hydrographical survey of many of the coasts and islands of the Indian and Chinese seas. It was soon after his return to Europe in 1805, that he communicated to this Society, through Mr. Cavendish, his very remarkable observations of the equatropical motions of the mercury in the barometer when at sea*; and contributed along with Captain Flinders, both by these observations and by other directions which he subsequently published, to make more fully known the importance of barometrical observations at sea, as affording indications of great or sudden atmospheric changes. Captain Horsburgh was soon afterwards appointed Hydrographer to the East India Company, with the usual judgement, and discrimination of the Directors of that Body, in the selection and rewarding of their officers; and it was in this capacity that he published not merely a great number of charts, but also "the East India Sailing Directory," the result of the unremitting labour of many years, and founded partly upon his own observations, and partly upon a very accurate examination and reduction of the vast hydrographical records which are in the possession of the East India Company; forming altogether one of the most valuable contributions that was ever made by the labours of one man to the interests of navigation. Captain Horsburgh was the author of other works connected with his favourite science, and he continued to devote him-

[* Capt. Horsburgh's paper here referred to was reprinted in Phil. Mag., First Series, vol. xxiii. p. 289.—Edit.]

self, until within a few days of his death, with almost unexampled industry, to those pursuits which had formed, throughout his whole life, the means by which he sought to benefit his countrymen and mankind.

Mr. William Blane was the author of a paper in our Transactions, written fifty years ago, on the production and preparation of Borax, which is brought from Jumlat in Thibet, over the Himalaya mountains into Hindostan.

Dr. David Hosack, of New York, was the author of a paper in our Transactions, published in the year 1794. It related to the explanation of the power which is possessed by the eye of adapting itself to different distances, which he attributed to the action of the external muscles of the eye, and not to the dilatation and contraction of the iris, nor to the muscularity of the crystalline lens, by which its convexity could be increased or diminished, a doctrine which had been promulgated in a paper by Dr. Thomas Young, in the preceding year. This subject is one of great interest, and has been very frequently agitated; and though an illustrious foreigner, M. Arago, has recently defended the theory of Dr. Young with great ingenuity and warmth, yet physiologists and anatomists are by no means agreed on the adoption of this or any other single explanation.

Mr. John Bell was Senior Wrangler at Cambridge in 1786, and a Fellow of Trinity College. Though labouring under physical disadvantages of no ordinary kind, and such as were apparently the most adverse to success in the public exercise of his profession as a lawyer, yet he conquered every difficulty and reached the highest eminence by his great acuteness and strength of mind, his extensive legal knowledge, and, not a little, likewise, by his sturdy integrity and love of truth, which he respected,—a rare virtue—, even in advocating the claims of a client. Mr. Bell, with an uncommon exercise of philosophy, retired from the active duties of his profession, whilst in the receipt of a splendid income from it, on the first warnings of the approaches of the infirmities of old age. He was a man of great liberality and kindness of heart, and remarkable for the steadiness of his attachment to a large circle of professional and other friends.

The Rev. William Lax, formerly Fellow of Trinity College, and Lowndes's Professor of Astronomy and Geometry in the University of Cambridge, was Senior Wrangler in the year preceding Mr. Bell, and throughout life one of his most intimate friends: he contributed two papers to our Transactions; one in 1796, on a subject of no great importance, and the other in 1809, on the method of examining the divisions of astronomical instruments, in the same volume which contained papers on similar subjects by Mr. Cavendish and Mr. Troughton. The method proposed by Mr. Lax, though very ingenious, requires great labour and time, and is inferior in accuracy and efficiency to that which was adopted by Mr. Troughton for tabulating the errors of the primary divisions of circular instruments. Professor Lax was the author of Tables to be used with the Nautical Almanack, and he had built a small observatory at his residence in Hertfordshire, where he occupied himself for the last

thirty years of his life with studies and pursuits connected with the advancement of astronomy.

Sir John Sinclair devoted nearly the whole of a very long and laborious life to pursuits and inquiries connected with the improvement of agriculture and the general benefit of his countrymen. He was a very voluminous author; and though different opinions may be entertained of the merit and usefulness of some of his later productions, the Statistical Account of Scotland which he originated, and arranged, will be a durable monument to his memory, presenting as it does a more complete and comprehensive record of the state of that kingdom at the period when it was compiled, than is to be found in the literature of any other country.

Dr. John Gillies, venerable alike for his great age and his amiable character, was the successor of Dr. Robertson, as the king's historiographer for Scotland: he was the author of a History of Greece and of the World from the conquests of Alexander to the age of Augustus, and he translated some of the Greek orators, the ethical, political and rhetorical treatises of Aristotle, upon whose speculative works generally he wrote a very enlarged commentary. He was a pleasing and popular writer, though not very profoundly acquainted with the great advances which have been made of late years in Germany and elsewhere in our knowledge of archæology and historical criticism.

Sir William Gell was well known as a topographical antiquary, and published works of great interest and research, some of them very splendidly embellished, on Pompeii, and on the modern, as illustrating the ancient topography of Troy, Ithaca, the Peloponnesus, Attica and Rome. He was a very accomplished artist and a man of great liveliness of conversation, and of very attractive manners. Sir William Gell was formerly Fellow of Emanuel College, Cambridge, and was attached, for some time, in the quality of Vice-chamberlain, to the late Queen Caroline. He spent the later years of his life, a victim to the gout and other infirmities, at Naples, in the neighbourhood of those remarkable ruins which he had so carefully and so beautifully illustrated, and which continued to supply him, from day to day, with fresh objects of interesting inquiry.

Dr. Warren, though one of the most distinguished physicians in this metropolis, contributed very little, by his writings, to medical or general literature: he was considered to be an accomplished classical scholar, and a man of very extensive acquirements: he was a strenuous vindicator of the character and independence of his profession, and though his manners were somewhat abrupt, and sometimes apparently uncourteous, yet he was a man of very warm affections, and greatly beloved and respected by a large body of friends.

Those to whom Dr. William Elford Leach was known in his happier days, when in the full enjoyment of health and reason, can best appreciate the great loss which the natural sciences and our national museum sustained by that melancholy visitation, which, like the hand of death, terminated his scientific labours. His enthusiastic

devotion to his favourite studies, his great knowledge of details, combined with no inconsiderable talents for classification, were eminently calculated to raise him to the very highest eminence as an original and philosophical naturalist. Though his career of research and discovery was prematurely cut short, yet we are chiefly indebted to him for the first introduction into this country of the natural system of arrangement in conchology and entomology, and for the adoption of those more general and philosophical views of those sciences which originated with Latreille and Cuvier. Dr. Leach was the author of a paper in our Transactions on the genus Ocythoë, to prove that it is a parasitical inhabitant of the Argonaut. He wrote several memoirs in the Linnæan Transactions; an excellent treatise on British Malacostraca: and he also contributed largely to the Zoological Miscellany, to Brewster's Encyclopædia and to the French *Dictionnaire des Sciences Naturelles.* He died of an attack of cholera on the 25th of August last, at the Palazzo St. Sebastiano, in the province of Tortona in Italy.

The last name which occurs in the melancholy list of our departed compatriot associates, is that of Dr. William Henry, to whom the science of chemistry generally, and of gaseous chemistry in particular, is under great obligations. He was the author of nine papers in our Transactions, many of them of great merit*; and his System of Chemistry is one of the best written and best arranged compendiums of that important and extensive science, which has been published of late years, whether in our own language or in any other. The Memoirs of the Manchester Society are chiefly indebted to him, in conjunction with Dr. Dalton, for the high character which they have so long maintained. Dr. Henry, like Dr. Wollaston, made the results of science, obtained by the most original and difficult researches, the foundation of a splendid fortune, and few persons have contributed more effectually, by their discoveries and exertions to the promotion of those arts and manufactures which form the foundation of the prosperity of a great commercial nation.

The names of the Foreign members whom the Society has lost during the last year are, André Marie Ampère and Antoine-Laurent de Jussieu, both of them members of the Académie des Sciences de France.

Mons. Ampère was born at Lyons in 1775, and made his first appearance in the scientific world in a short work which showed con-

[* Of these nine papers by Dr. Henry in the Philosophical Transactions, seven have been given entire in the Philosophical Magazine, together with an abstract of another. Dr. H.'s *Account of Experiments to decompose Muriatic Acid,* will be found in Phil. Mag., First Series, vol. vii. p. 211; an abstract of his paper on the Absorption of Gases by Water, (from the pen, we believe, of Sir H. Davy,) in vol. xvi. p. 89; his *Description of an Apparatus for analysing Inflammable Gases, with Experiments on the Gas from Coal,* in vol. xxxii. p. 277; *Experiments on Ammonia,* in vol. xxxiv. p. 369; *Analysis of British and Foreign Salt,* in vol. xxxvi. p. 106; *Additional Experiments on Muriatic Acid,* in vol. xl. p. 337; *On the aëriform compounds of Charcoal and Hydrogen,* in vol. lviii. p. 90; and *On the action of finely divided Platinum on Gaseous Mixtures,* in vol. lxv. p. 269.—Edit.]

siderable command of analysis, entitled *Considérations sur la Théorie Mathématique du Jeu*, in which the question of the safety of habitual and indefinite play, either against a single person of greater fortune, or indifferently against any number of persons, even when the game is perfectly fair and equal, is discussed and solved, and its result exhibited in a form full of warning to those by whom gaming is pursued as an occupation, in which success or failure is considered as the gift of fortune, and not the inevitable result of calculation. M. Ampère was subsequently appointed Professor of the Polytechnic School, and published memoirs on the integration of partial differential equations, and on other subjects, which show a profound knowledge of some of the most refined and difficult artifices of analysis: to him likewise we are indebted for memoirs on the Mathematical Theories of Electro-magnetic Currents, which are remarkable for the skill and ingenuity with which the powers of analysis are brought to bear on subjects apparently the most remote from their operation. His inquiry into the equation of Fresnel's wave surface is more remarkable as an example of resolute perseverance than of success, and his last work, on the Philosophy of the Sciences, showed him to be much less happy in his metaphysical, than in his physical and analytical speculations. M. Ampère was a man of great simplicity of character, and his extraordinary fits of absence of mind were not unfrequently made the subject of much innocent amusement. He took no part in the cabals and jealousies which too frequently disturb the peace of the world of science, and he was universally respected and beloved for his great integrity and the kindness of his affections.

Antoine-Laurent de Jussieu, a name singularly illustrious in the annals of botanical science, was born at Lyons in 1748. He was nephew to the great Bernard de Jussieu, under whose auspices he was first introduced into the scientific world of Paris, and appointed, at a very early age, demonstrator of botany in the Jardin du Roi. After this appointment, though originally destined for the profession of medicine, he devoted himself almost exclusively to the study of botany, more especially with a view to the establishment and developement of the natural system of arrangement, a very bold and successful approximation to which had been effected by his uncle in the distribution of the plants in the Garden of the Trianon.* He succeeded his uncle as administrator of the Jardin des Plantes in 1779, and published two memoirs of great originality and importance on the relative value of characters in the distinction of the genera and orders of plants. In the year 1789 he published his great and truly classical work entitled *Genera Plantarum secundum Ordines naturales disposita*, which caused a total revolution in the science of botany. To the modification and extension of the views contained in

* This arrangement, made in 1759, is given by his nephew at the conclusion of his introduction to his great work, published in 1789: though extremely imperfect and in many respects erroneous, it was founded upon just principles, and was in almost every respect superior to those which had been proposed by Linnæus and by Tournefort. [See our last number, p. 38.—Edit.]

that work, rendered necessary by new observations and by the vast accession of new genera and orders, brought from the tropics, South America, Australia, and elsewhere, he devoted the remainder of his life. His later memoirs, many of which are of great value, are chiefly contained in the *Annales*, and subsequently in the *Mémoires du Museum d'Histoire Naturelle*. M. de Jussieu was a man of very simple manners and amiable character, of a social and affectionate temper, and a perfect stranger to scientific jealousies and intrigues. He attained to an extreme old age, and had the happiness of witnessing the almost universal adoption of that system of botanical arrangement, the establishment of which had formed the great object of the labours of his life.

XXXI. *Intelligence and Miscellaneous Articles.*

ON THE REDUCTION OF METALS BY ELECTRICITY.

M. BECQUEREL, on presenting some electro-chemical apparatus to the Académie Royale des Sciences of Paris, by the aid of which he had been able to effect the immediate reduction of silver, lead and copper, stated that, without the intervention of mercury, by constructing an electro-chemical apparatus with iron, a saturated solution of common salt, and an ore of silver, properly prepared, he had extracted from the latter the silver which it contained, under the form of crystals. The minerals on which the experiments were made were the ores raised in Columbia and the ore of Allemont. The same method has also been successfully employed to extract from the copper pyrites of Chessy, near Lyons, the silver which it contains, without affecting the copper. It is only from the argentiferous galenas that it is difficult to extract the silver. When a mineral like that of Allemont contains many metals, as lead, copper, &c., each of these metals is separately reduced and at different times, so that the separation is easily effected. From this it results that the ores of lead and copper may be treated in the same manner as those of silver, but with much less facility, because of the different degrees of oxidation which they acquire, and the compounds which they form during roasting. M. Becquerel is at present occupied with further researches on the extraction of metals, but deemed it proper, for the interests of science, to make known to the Academy the principle by means of which he had been able to extract some metals, particularly silver, from their respective ores.—*L'Institut*, Mars 2, 1836.

ON A SIMPLE METHOD OF OBTAINING SPONGY PLATINA.

To obtain spongy platina, M. Döbereiner fuses crude platina with twice its weight of zinc, and treats the alloys, when powdered, first with dilute sulphuric acid, and then with nitric acid also diluted, to oxidize and dissolve all the zinc, which, contrary to theory, takes place but slowly, even with heat: there is thus obtained an insoluble greyish black residue of finely divided but impure platina, which,

when properly purified by a solution of potash and water, acts like spongy platina, and possesses such remarkable oxidizing properties, that it not only converts formic acid into carbonic acid, and alcohol into acetic acid, but even the osmium which it contains into osmic acid. This latter is soon formed when the powder of platina is dried, and may be separately obtained by distilling the platina with a little water. It is immediately reduced by alcohol, and consequently is not obtained amongst the products of the oxidation of this liquid.

This method, recommended thirty years since by Descotils, is an excellent one for the preparation of platina in a state of minute division, and is chiefly applicable in working large masses of native platina like those of the Oural, and also for the preparation of the spongy platina employed for absorbing oxygen and for the production of acetic acid.—*Jour. de Pharmacie*, July 1836.

ON THE DECOLORIZING COMBINATIONS OF CHLORINE.

M. Martens, in a former memoir on this subject already published, offered experiments and arguments which tended to show that the decolorizing chlorides ought to be regarded as feeble combinations of chlorine and basic oxides. Some short time after this M. Balard, of Montpellier, again raised a doubt on the question, and by his discovery of hypochlorous acid and of decolorizing hypochlorites, appeared to have proved that the opinion M. Martens had advocated was incorrect; that is to say, according to M. Balard, these bleaching compounds ought to be considered as mixtures of hypochlorites and chlorides, agreeably to the hypothesis of Berzelius. Returning to this first memoir and comparing the properties of hypochlorites with the decolorizing chlorides, M. Martens has arrived at results which, in his opinion, do not permit us to confound the two compounds, and which serve to confirm the former view of their composition as chlorides of oxides. We now proceed to the principal facts contained in the present memoir, and which the author considers he has established by his experiments.

1st. The binoxide of chlorine of some chemists must be considered as an acid under the name of *chlorous acid*: it forms compounds with the compound alkaline oxides (oxides alcalins composés (alkalis ?)), which may be called *chlorites*, and which are decomposed by almost all the acids, with effervescence and disengagement of chlorous acid.

2nd. The chlorites may be obtained in the solid state by evaporation, without decomposing, taking care that they have excess of base, or, rather, that they possess an alkaline action. When they are saturated so as to indicate neutrality with litmus paper, their solutions, when they are concentrated or evaporated, are decomposed into chlorates and chlorides, like the chlorides of oxides, with the difference of affording in proportion much more chlorate than the latter.

3rd. The chlorites possess an extremely powerful decolorizing and oxidizing action, like the chlorides of oxides and the hypochlorites of M. Balard. Those which are not saturated with chlorous acid

do not destroy colour, except by the intervention of an acid, but the others decolorize instantly, like free chlorine.

4th. The chlorites, even when mixed with metallic chlorides, possess all the characteristic properties which distinguish them from the decolorizing chlorides of oxides; and, amongst others, that of the disengagement of chlorous acid, on the addition of acids, instead of chlorine, which, under these circumstances, is evolved from the chlorides of oxides.

5th. The chlorites are analogous to the hypochlorites of M. Balard as regards their decolorizing and oxidizing power, but are much more stable.

6th. Although the hypochlorites, mixed with metallic chlorides, disengage only chlorine by the addition of acids, as M. Balard has rightly observed, and are in this respect analogous to the decolorizing chlorides of oxides, it need not necessarily be inferred that their chemical constitution is the same, since hypochlorous acid itself, acting on a metallic chloride, evolves only chlorine. We may, moreover, compare this phænomenon to that of many other analogous chemical actions, and, amongst others, to the fact stated long since by Gay-Lussac, that a mixture of iodate and iodide of potassium, when acted on by even the weakest acids, disengages iodine; that also which is presented to us by a mixture of chlorate and chloride of potassium, which readily decomposes with the simultaneous evolution of chlorous acid and chlorine, by the action of acids so much diluted that they had no action on either of the compounds of the mixture taken separately. All these actions may be easily represented and explained by atomic formulæ.

7th. By distilling the chlorides of soda and potash supersaturated with chlorine, hypochlorous acid is produced, the residue being a neutral metallic chloride. This is a new and simple method of obtaining this acid, which has hitherto only been obtained by the action of certain insoluble metallic oxides on chlorine with the intervention of water.

8th. Red oxide of mercury does not present so many advantages for the preparation of hypochlorous acid, because it may form an insoluble oxichloride, which renders the chloride of oxide of mercury, though easy to obtain, unstable, and readily causes its conversion into hypochlorous acid and an insoluble oxichloride; so that this chloride of an oxide may give rise to hypochlorous acid without being supersaturated with chlorine.

9th. The decolorizing chlorides of potash and soda, when they have excess of base, may be evaporated without decomposing, and even heated to 212° Fahr., without losing their decolorizing power, which is not the case with the hypochlorites, and indicates a different composition.

10th. The production of hypochlorous acid, by the distillation of the chlorides of potash and soda, supersaturated with chlorine without an alkaline residue, can be but little understood by the hypothesis that these decolorizing chlorides consist of hypochlorites mixed with metallic chlorides, although it may be easily deduced

from the composition originally assigned to the chlorides of oxides. —*L'Institut*, Juillet 13, 1836.

ON THE ACTION OF ANHYDROUS SULPHURIC ACID ON SOME METALLIC CHLORIDES.

M. Rose, in the course of his memoir, notices the researches of L. Gmelin, which have shown that anhydrous sulphuric acid decomposes common salt in a very different manner to what the hydrated acid does, for although the first converts it into sulphate of soda like the second, yet in the first case the sodium is oxidized at the expense of the sulphuric acid, and during the decomposition sulphurous acid is disengaged along with chlorine. M. L. Gmelin, and after him MM. Sertürner and Döbereiner, made all their experiments by directing the vapour of anhydrous sulphuric acid upon heated common salt. The result is, however, very different when the vapour of the anhydrous acid is passed over chloride of sodium finely powdered and placed in a vessel which is kept cool by a freezing mixture. In this case the acid vapour is rapidly absorbed by the metallic chloride without decomposing it. The whole is converted into a transparent mass, which is at first soft, but hardens by degrees; it emits no fumes, not affording the slightest traces of hydrochloric acid, of chlorine, nor of sulphurous acid. This mass, which is composed of anhydrous sulphuric acid and chloride of sodium, decomposes when heated, and is converted into sulphate of soda, with the disengagement of chlorine and sulphurous acid gas.

The chloride of potassium and hydrochlorate of ammonia act in the same manner with the vapour of anhydrous sulphuric acid as common salt, except that the ammoniacal salt absorbs it with even greater rapidity than the other two. If we heat the compound formed by the salammoniac and the anhydrous acid, hydrochloric acid gas is at first disengaged, and afterwards the phænomena occur which accompany the sublimation of sulphate of ammonia.

If these compounds of anhydrous sulphuric acid and a chloride are moistened with a few drops of water, abundance of hydrochloric acid gas is immediately disengaged, and when also they are exposed to a damp atmosphere they immediately begin to decompose, with the evolution of the same gas.

All the metallic chlorides do not equally unite with anhydrous sulphuric acid; thus, it cannot be combined with the chlorides of barium and copper in their anhydrous state. On the contrary, anhydrous sulphuric acid unites with some equally anhydrous salts; amongst others, with the nitrate, and even, although slowly and with difficulty, with the sulphate of potash. The most important of these compounds is that of anhydrous acid, with sulphate of ammonia, likewise anhydrous, which is always simultaneously formed in the preparation of these latter salts*, and which prevents us obtaining them pure and in quantity.—*Jour. de Chimie Médicale*, Oct., 1836.

* So in the French.

ARTIFICIAL FORMATION OF CRYSTALLIZED IRON PYRITES.

This process of M. Wöhler consists in slowly heating in a glass flask, or other convenient vessel, peroxide of iron, sulphur, and hydrochlorate of ammonia, intimately mixed, until all the ammoniacal salt is sublimed, suffering the mass to cool slowly, and afterwards washing with water; there will be found at the bottom of the vessel heavy octohedra and tetrahedra, of a yellow colour, which are identical with the common crystallized pyrites. The larger the mass of the materials employed, the larger and more perfect are the crystals obtained.—*Jour. de Pharmacie*, Oct., 1836.

ON THE DEGREE OF COLD PRODUCED BY SOLID CARBONIC ACID.

M. Thilorier has invented an apparatus of a very simple construction, (which, however, we regret to say, is not described,) by which masses of from two to three hundred grains of solid carbonic acid can be quickly and œconomically obtained.

The acid obtained by this apparatus resembles compact snow. In his former experiment on the degree of cold produced by this substance, the author directed a jet of liquid carbonic acid on the bulb of a thermometer, &c., but the facility with which the solid acid is now obtained in abundance has allowed M. Thilorier to use a preferable method of manipulation. The bulb of a thermometer was placed in the midst of a small portion of solid carbonic acid, and at the expiration of about two minutes the thermometrical index became stationary, having sunk to – 162° Fahr. A few drops of æther or alcohol poured on the solid acid do not indicate any appreciable alteration of temperature. The æther forms a half-liquid mixture of about the consistence of melting snow; but the alcohol, uniting with the solid acid, congeals, forming a hard, brilliant and semi-transparent ice. This congelation of anhydrous alcohol does not occur unless it is mixed with the acid, for when the alcohol is poured into a silver tube, and this plunged into solid carbonic acid, no change is produced.

The mixture of alcohol and solid carbonic acid begins to melt at –153° Fahr., and, commencing at this point, the temperature does not vary; thus affording at this extremity of the thermometric scale a point as invariably fixed as that indicated by melting ice.

When 150 or 180 grains of mercury are placed on a small and concave portion of solid carbonic acid, it solidifies in the course of a few seconds, and continues so as long as any solid carbonic acid remains, that is, for 20 or 30 minutes, when the weight of the acid is about 140 grains.

Although the addition of æther or alcohol does not increase the degree of cold, yet it enables the solid acid to moisten, and to adhere more closely to, the surfaces of bodies, thus greatly increasing its refrigerating power. A portion of carbonic acid, on the addition of a few drops of alcohol or æther, will solidify from 15 to 20 times its weight of mercury, thus affording an easy and elegant method, far superior to any yet used, for the solidification of mercury.

The specific gravity of liquid carbonic acid is so variable that, from 32° to 86° Fahr., it runs through the scale of density from water to the æthers. Its dilatibility, the pressure and weight of its vapour, are four times greater than that of atmospheric air; its capillarity, and particularly its compressibility, are a thousand times greater than that of water. From these facts the author has been led to a uniform and constant law, by which he connects phænomena which at first sight seem to be quite independent of each other.—*L'Institut*, Oct. 5, 1836.

MELLITIC ACID.

MM. Liebig and Pelouze, in endeavouring to determine the composition of mellitic acid, made some experiments on the salt it forms with oxide of silver, which have led them to think that this acid should be considered as a hydracid, their experiments confirming in this respect the views of M. Dulong on oxalic acid.

Mellitate of silver dried in vacuo over sulphuric acid contains hydrogen, which it does not lose at a temperature below 356° Fahr.; it is then expelled as water, and the salt changes its colour. This is the case with no other salt of silver, as they are all anhydrous. In this case the formation of water seems to be owing to the reduction of the oxide, and not to a simple volatilization of water existing in the salt. According to known analyses, mellitic acid contains 3 equivalents of oxygen. MM. Liebig and Pelouze, however, suppose it contains 4 of oxygen and also 1 of hydrogen; that this hydrogen enters into the constitution of all the mellitates, except mellitate of silver, which has been heated to 356°, regarding this last salt as a compound of metallic silver with the radicle of the hydracid.—*L'Institut*, Oct. 5, 1836.

METEOROLOGICAL OBSERVATIONS FOR DECEMBER 1836.

Chiswick.—Dec. 1, Foggy. 2. Rain: stormy. 3. Rain. 4. Overcast: rain at night. 5. Overcast: clear. 6. Cloudy and fine. 7. Stormy showers. 8. Showery: clear with lightning at night. 9. Cloudy and cold. 10. Clear and frosty: slightly overcast. 11. Slight fog: rain. 12. Rain: stormy at night. 13, 14. Cloudy and cold. 15, 16. Fine. 17. Slight frost: overcast. 18, 19. Fine. 20—22. Foggy. 23. Cloudy. 24. Clear and cold. 25—27. Snowing and stormy. 28. Overcast. 29. Snowing. 30. Hazy: snowing: cloudy and cold. 31. Hazy: cloudy and cold.

The heavy snow storm, which, at London, commenced on the 25th, is perhaps the most remarkable of any recorded at the same period of the season. It appears to have been general, not only over Britain, but also over a great part of Europe. A fall of snow is mentioned as having been experienced at Bilboa in Spain, on the night of the 24th.—R. THOMPSON.

Boston.—Dec. 1. Fine. 2. Cloudy. 3, 4. Fine. 5. Rain. 6. Fine. 7, 8. Fine: rain early A.M. 9—11. Fine. 12. Cloudy: rain A.M. 13—15. Fine. 16. Fine: rain early A.M. 17. Fine. 18, 19. Cloudy. 20—23. Fine. 24. Cloudy: snow P.M. 25. Fine: snow A.M. and P.M. 26. Snow. 27. Cloudy. 28. Snow. 29. Cloudy. 30. Snow. 31. Cloudy: melted snow.

N.B. The 24th, 25th and 26th an immense fall of snow, which was drifted in some places ten feet deep. The mail twenty hours behind the usual time.—S. VEALL.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary; by Mr. THOMPSON at the Gardens of the Horticultural Society at Chiswick, near London; and by Mr. VEALL at Boston.

Days of Month. 1836. Dec.	Barometer.				Thermometer.						Wind.			Rain.			Dew-point.
	London: Roy. Soc. 9 A.M.	Chiswick.		Boston. 8½ A.M.	London: Roy. Soc.			Chiswick.		Boston. 8½ A.M.	London: Roy. Soc. 9 A.M.	Chisw. 1 P.M.	Bost.	London: Roy. Soc. 9 A.M.	Chisw.	Boston.	Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
		Max.	Min.		Fahr. 9 A.M.	Self-registering. Min.	Self-registering. Max.	Max.	Min.								
☾ Th. 1.	29·980	30·180	29·989	29·54	41·3	39·0	48·7	50	38	34	SW.	SW.	calm	·325	...	...	42
F. 2.	30·012	30·096	29·745	29·47	49·0	40·4	51·7	53	38	46	S.	SW.	NW.	...	·04	...	42
S. 3.	29·772	29·911	29·671	29·12	49·3	48·5	54·2	55	50	47	SSW.	SW.	W.	·036	·06	...	44
☉ 4.	29·816	29·876	29·767	29·12	53·7	48·6	55·5	56	52	55	SW.	SW.	W.	·161	·14	...	47
M. 5.	29·867	29·949	29·866	29·24	54·2	51·3	55·7	55	46	50	WSW.	W.	calm	·125	·02	·12	48
T. 6.	29·998	30·010	29·861	29·42	51·9	47·2	53·7	54	48	43	S.	SW.	calm	...	...	...	48
W. 7.	29·535	29·579	29·446	28·93	50·6	49·3	52·7	55	41	49·5	SSW.	S.	calm	·055	·52	·07	47
● Th. 8.	29·229	29·252	29·124	28·67	46·2	42·5	46·5	47	36	43	SW.	W.	calm	·380	·04	·02	43
F. 9.	29·069	29·097	28·963	28·62	41·3	38·2	43·6	45	33	37	SSW.	SW.	calm	·036	·02	...	41
S. 10.	29·213	29·432	29·190	28·75	36·5	34·6	42·0	43	30	37	SSW.	W.	NW.	...	...	...	37
☉ 11.	29·586	29·703	29·620	29·15	38·0	35·0	40·2	41	26	34	SW.	SW.	calm	...	·17	...	36
M. 12.	29·584	29·603	29·298	29·20	38·7	33·5	50·3	51	40	35	ESE.	SW.	calm	·119	·14	...	35
T. 13.	29·241	29·268	29·244	28·73	47·7	38·2	49·3	51	36	47	SSW.	SW.	calm	·066	...	·11	42
W. 14.	29·295	29·668	29·321	28·78	38·6	37·4	45·2	52	35	38	SSW.	SW.	calm	...	·02	...	38
☽ Th. 15.	29·967	30·055	29·796	29·54	38·0	37·0	44·6	45	30	36	SW.	W.	calm	...	·16	...	37
F. 16.	29·728	30·051	29·780	29·24	39·3	37·0	44·0	40	31	38	S.	NW.	W.	·183	...	·12	36
S. 17.	30·124	30·153	30·102	29·65	37·2	35·6	49·0	52	39	38	SW.	SW.	calm	...	·01	...	37
☉ 18.	30·095	30·153	30·110	29·57	49·2	36·4	51·2	52	43	50	SSW.	S.	calm	...	...	...	39
M. 19.	30·168	30·196	30·149	29·61	47·6	45·8	49·9	52	42	46·5	SSW.	SW.	calm	...	·05	...	41
T. 20.	30·208	30·294	30·205	29·70	46·5	45·7	46·2	45	39	40	SW.	SW.	calm	·058	...	...	43
W. 21.	30·279	30·342	30·304	29·83	42·8	39·3	47·0	46	38	38	SSW.	NW.	calm	...	...	...	41
T. 22.	30·378	30·399	30·092	29·91	42·8	41·9	48·0	48	34	39	SW.	SW.	calm	...	·09	...	40
○ F. 23.	29·857	29·886	29·702	29·40	39·7	38·9	43·7	46	31	36	N.	N.	calm	·102	...	...	39
S. 24.	29·711	29·841	29·756	29·47	34·3	32·7	38·2	36	26	31	W.	N.	calm	...	...	...	33
☉ 25.	29·766	29·826	29·707	29·58	28·3	27·4	34·0	35	28	29	N. var.	NE.	calm	...	...	...	27
M. 26.	29·580	29·635	29·596	29·50	29·7	29·2	32·5	35	30	30	SW.	NE.	E.	...	...	...	27
T. 27.	29·578	29·826	29·657	29·37	32·5	31·9	33·0	33	28	34	NE. var.	NE.	E.	...	...	...	28
W. 28.	29·885	29·997	29·958	29·63	33·3	29·4	31·8	35	26	32	NW.	NE.	calm	...	...	...	27
T. 29.	29·998	30·065	30·028	29·79	31·9	29·7	33·2	35	27	33	NE. var.	NE.	calm	...	...	...	28
F. 30.	30·053	30·193	30·113	29·80	30·2	29·2	29·8	32	26	30·5	N.	N.	calm	...	...	...	27
☾ S. 31.	30·212	30·474	30·337	29·95	30·0	28·2	32·3	32	27	30	NE.	NE.	calm	...	...	·78	27
	29·799	30·474	28·963	29·36	41·0	38·0	44·4	56	26	38·9				Sum 1·646	1·48	1·22	37·6

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XXXII. *Notice of a Vein of Bituminous Coal in the Vicinity of Havana, in the Island of Cuba.* By Richard Cowling Taylor, *Esq., F.G.S., and* Thomas G. Clemson, *Esq.**

THE substance of the following article was recently read to the American Philosophical Society, and its authors are desirous of extending a knowledge of its details by means of the Philosophical Magazine.

The bituminous coal mine of Casualidad is situated about three leagues east of the city of Havana, and on a main road (*Camina Real*) leading to the city of Guanabacoa, from which place it is distant six miles, and from the sea, at a place of embarcation, only two miles.

From Guanabacoa eastward, the surface of the country is undulating, and partakes of those characters which are so marked elsewhere in this island, where the serpentines and the euphotides are the predominant rocks. In leaving Regla, on the south side of the bay of Havana, the euphotides,—which Baron von Humboldt has described here, and which we found also to exist with similar characters in the district of Holguin, and for many miles throughout the long chain of barren savanas which stretch along the north-eastern portion of the island, and contain fine veins of copper,—are here evidently the most prevailing rocks. They occupy a breadth of about two leagues, within which area the city of Guanabacoa and the adjacent petroleum springs are situated.

When within half a mile of the coal vein in question, a

* Communicated by the Authors.

fragile, dirty-gray-coloured, argillaceous rock succeeds to the serpentine and alternates with the euphotides.

Towards Guanabacoa, and indeed throughout a large portion of the island of Cuba, the prevailing course or strike of the rocks is about east and west; but in the vicinity of the coal we unexpectedly found that the direction changed to north and south.

The coal vein of Casualidad is visible at opposite extremities of an excavation thirty feet deep, of a quadrangular form, descending on one side by steps cut in the soft rock or clay which bounds the coal on either side. This soft rock is fragile, incoherent, distorted, of a yellowish green colour, like the prevailing euphotides, of which it is a variety. A few feet to the eastward of the vein there occurs a hard blue siliceous rock, containing small cavities that are partially filled with a leek-green mineral which we conceive to be a variety of serpentine.

In close connection with the above, a beautiful diorite occurs, the base of which is petrosiliceous, tinged with green, which colour is caused by a mixture of serpentine. This rock is very hard, and has a highly crystalline structure. It crops out at several points.

The siliceous rock, the diorites, and the euphotides alternate the one with the other. The two first mentioned are in much less proportion than is the third, which is by far the most predominant rock of the country.

All these are highly inclined, and frequently are vertical; their direction, as before stated, being north and south, in the neighbourhood of the mine.

Appearance of the Coal Vein. — The vein commences or crops out immediately under the thin alluvial soil of the surface, and follows an irregular, but nearly perpendicular direction downwards, so far as it has been traced, and as shown in fig. 1.

It is visible to the depth of full thirty feet; but the bottom of the excavation being covered with mud, which had been washed in during the rainy season, we could not readily define the breadth of the vein there; but it was stated by the overseer to be nine feet thick. From even this small opening many tons of pure coal had been extracted, and were deposited in a large building adjacent.

On the north side of the excavation the vein is solid; having a thickness gradually increasing to four feet at the depth of twenty feet. The coal lies in parallel horizontal layers, of from one to four inches in thickness, across the vein. Sometimes these layers appear to have their horizontal position par-

tially disturbed, particularly near the outer edges, in which case they were slightly curved. Where an accidental derangement of the vein has taken place, this curving up of the outer edges or extremities of the planes of stratified bituminous matter is particularly observable. Near the walls of the vein, the laminæ of the coal for a few inches in depth are deflected, as if they had been pressed by the sides or walls. Here the structure becomes bacillary, and the coal on the slightest effort divides into irregular polyhedrons. The surface of this coal, when detached from the walls, instead of being smooth or covered with any kind of bituminous shale, is rough, and presents a bacillo-fibrous appearance, similar to the structure observed in arragonites and other fibrous minerals. Two or three small branches or *filons* are seen passing from the main vein at about the depth of twenty feet, occupying small fissures in the surrounding rock. These branches all rise towards the surface, but at different angles.

Fig. 1.

Section of the Coal Vein to the depth of 30 feet at the Mine of Casualidad, in the Island of Cuba, 10 miles east of Havana.

On the south side of the opening the coal, in rising towards the outcrop, parts off into two separate veins, longitudinally, for an uncertain space; and is apparently more disseminated through this rock than on the north side.

With regard, therefore, to the mine of Casualidad, we have

here, in the strictest sense of the term, A COAL VEIN, unlike any we have before witnessed in any part of the world. It is distinguished from the ordinary deposits of coal, in as much as those occur in distinctly stratified beds, and almost invariably exhibit abundant traces of organic remains, for the most part of vegetable origin; whereas we have before us a geological phænomenon of no common occurrence, yet whose origin seems sufficiently intelligible. It was evidently, originally, an irregular open fissure, terminating upwards in a wedge-like form; having various branches, all of which have been subsequently filled with carbonaceous matter, as if injected from below, and that not by slow degrees or by an infinite succession of depositions, but suddenly and at once.

This coal is wholly unaccompanied by traces of vegetable remains, or by those beds of bituminous or other shales which almost invariably envelop, cover, or accompany ordinary coal seams, whether in secondary or transition formations. The layers or transverse seams of which we have spoken appear to maintain a horizontal position, that is to say, at right angles to the vein, and when otherwise, the result is accidental, or produced by an after cause. This fact, together with the bacillo-fibrous structure observed, where the coal is in contact with the walls, are among the reasons which lead us to lean towards the supposition that the fissure was charged or filled at once, and that these characteristics are the result of the carbonaceous matter having passed to a more solid state in its present position.

It would be rash to pronounce an opinion on the presumed extent of this deposit, or to speculate on the probable magnitude of the vein, below the point at which it is visible at the depth to which we have had access. But if the vein continue to enlarge downwards, in the same proportion as it has augmented in the first thirty feet, or even if it holds the present breadth of nine feet, the quantity of this mineral must be very great, and will prove a highly acceptable discovery, so near the precincts of a great and flourishing city, so convenient to the Embarcadero on the sea-coast, and in the midst of a district from which nearly all the timber for fuel has been long since removed.

Quality.—This coal is unusually light, its specific gravity being commonly not more than 1·142. Two experiments upon heavier specimens, (for the density is by no means uniform,) gave 1·189 and 1·197.

It is perfectly jet black; having a resplendent lustre, which is much greater in one direction, or under one aspect, than in the other; and it divides into parallel layers in the mass. The

surfaces of the divisions or partings of the coal are brilliantly shining. Its cross fracture is rough, and has a glimmering pitchy appearance.

We have now to advert to an external character which is very common, and which, in fact, is of constant and universal occurrence in this combustible; a feature which distinguishes it from all other coals which have come to our knowledge in any quarter of the globe. Its horizontal fracture or surface is marked, as shown in figure 2, by numerous concentric, or, more properly speaking, excentric rings of various sizes, from a twentieth part of an inch to a foot in diameter. They are perfectly regular and uniform in shape, smooth, shining, conchoidal, resembling the impressions made by a seal in black wax, or when first seen, appear like the casts of the flat valves of some shells.

Fig. 2.
Form of the Impressions on the laminæ of the Coal of Casualidad.

This coal is exceedingly friable, breaking into small fragments under the hammer. Its powder is brown, and when pressed under the pestle, takes a polish, like certain resinous substances. It burns with much flame and a great deal of smoke; melts, and gives a light voluminous cake, which, when incinerated, leaves comparatively a small proportion of cinders or ashes.

The following analysis, which was made by one of us, gave per cent. as follows:

Volatile matter (gas, &c.)	63·00
Carbon	34·97
Ashes and cinder	2·03
	100·

The foregoing examination of this bituminous coal (for we see no reason to separate it from combustibles of that class) fixes definitively the respective proportions of its component parts; consequently it determines the applications to which that combustible would be the best adapted. Its quality of burning with a long licking flame gives it obvious advantages for evaporating, heating surfaces, &c., over many descriptions of fuel which contain a smaller quantity of volatile matter.

For the generation of steam power, for boiling or concen-

trating the juice of the sugar-cane, or for the manufacture of gas, this coal is singularly well adapted. As it contains no sulphuret of iron, the gas manufactured would be free from that very deleterious portion or admixture, which is so difficult to separate from those gases usually manufactured from bituminous coals containing sulphur. It might also be employed with advantage in manufacturing lamp-black (*Noir de fumée*).

Quantity.—As we have no knowledge of coal being ever before found in formations similar to those in which the mine of Casualidad occurs, no opportunity is afforded us of reasoning from analogy, and from the experience derived from the exploration and working of similar deposits. It will therefore be admitted, that whatever observations we might be induced to hazard concerning the extent of carbonaceous matter existing here, they would necessarily be founded more or less upon conjecture.

The outcrop of this singular vein was accidentally discovered where the public road winds down the point of a small ridge, and was worn down sufficiently deep to expose the coal and attract attention.

In whatever way we may account for the origin of this remarkable coal deposit, in a rock of this age, we must be led to view it, in some measure, in connection with the petroleum which is found in the rocks of this region. We observed it in a liquid form, filling cavities or cells in veins and masses of chalcedony, a few yards only from the coal vein; and whilst breaking with the hammer fragments of euphotide, serpentine, and various rocks in this vicinity, during a hot day, we perceived a strong odour of pitch or tar, arising after every blow.

The petroleum springs which rise from fissures in the serpentine at Guanabacoa, two leagues distant westward, have been known for two centuries.

Round a great portion of the Bay of Havana, asphalt is still collected at low water, under the name of *Chapapote*, and is employed in the manner of tar, for paying vessels. As this substance is remarkable for yielding a dense column of black smoke, when ignited, it has frequently and at various times been had recourse to for the purpose of making signals along the neighbouring coast of the island. It is matter of history, that Havana was originally called by the discoverers and early occupiers of this part of Cuba by the name of *Carine*, because there they careened their ships and pitched them with the natural tar they found washed on the shores of this beautiful bay.

The position which we have described in the foregoing

notice, is not the only one in the island of Cuba where this remarkable variety of coal exists. Nearly contemporaneously with the discovery of the coal of Casualidad, it has been observed about midway between the cities of Matansas and Havana, not far from the sea-coast. If this be not a prolongation, at the distance of six leagues, of that we have here described, it is an additional evidence of the great prevalence of bituminous matter in the serpentine and euphotides of Cuba. The vein at this position has not yet been worked, nor have mining operations been arranged at either position. Some barrels of the coal from near Matansas have been lately received at Philadelphia as specimens.

We are not aware that any other of the West India islands contain coal in sufficient quantity to be worked. In Jamaica, it appears, on the authority of Mr. De la Beche, coal exists in veins of an inch or two in thickness, occurring stratified with the usual coal-measures and carboniferous rocks; but these veins are too insignificant to be worth mining.

Of the geology of St. Domingo we know very little, and shall probably remain ignorant for a long time to come.

It were an interesting fact, if it be as we conceive, that this is the first discovery within the tropics, in this part of the globe, of workable veins of remarkably pure coal.

Philadelphia, Dec. 20, 1836.

XXXIII. *On* Mr. Peter Nicholson's *Rule for the Construction of the Oblique Arch.* *By* Charles Fox, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

HAVING seen in your last Number (p. 75) a letter extracted from the Newcastle Journal, addressed by Mr. Henry Welch to Peter Nicholson, Esq., in which he states "the propriety of setting the public right as to whom the merit is due, for a proper and *certain* rule for the construction of the oblique arch," and subsequently says, "I think that there can be no doubt, that your claim to the rule for the proper formation of the stones is prior to that of Mr. Fox, and I have yet to learn that any rule exists by which the oblique arch can be so truly built as the one which you have published;" I feel, therefore, bound in justice to myself, briefly to notice it, and also because the principle laid down in my former paper (*vide* vol. viii. of this Magazine, p. 299) does not ap-

pear to have been clearly understood by the writer of the letter in question.

The two points to be determined are, whether my rule is identical with Mr. Nicholson's; and if not, which is the better of the two.

That they are not identical I hope will be evident from the following facts; and which is the better of the two must be left to the judgement of your readers.

No one would for a moment hesitate to acknowledge the obligations which practical men are under to that highly talented individual Mr. Peter Nicholson; but on referring to his Treatise on Masonry and Stone-cutting (plate 17), it will at once appear that the intrado is the only surface developed, and the approximate line being laid down upon it, all the courses are drawn at right angles to that line; the courses therefore are drawn with reference to the intrado only. Now my plan is to make use of neither *intrado* nor *extrado* for this purpose, but to lay down all the courses upon a supposed intermediate surface: the two plans are therefore not identical.

Now the reasons why I conceive my plan to possess advantages superior to Mr. Nicholson's are these. On referring to vol. viii. of this Magazine, Plate III. fig. 6, it will be seen that the angles of the courses, as shown in the developed intrado and extrado, differ very considerably, even more than 16°: it will therefore be obvious, that if the angle of the intrado is adopted for the rule (as by Mr. Nicholson) then the angle of the extrado will be wrong; and again, if the angle of the extrado were adopted for the rule, then the angle of the intrado would be incorrect; for in either case the thrust would not be in the direction of the abutment, which in every arch is an essential point.

Now to use my own words (vol. viii. p. 302), "It is evident from fig. 7, that if spiral planes are considered as composed of spiral lines, placed at various distances from the centre of the cylinder, each of these lines will form a different angle with the axis; and therefore, as an arch has always some thickness, that although we have the inner edge of the spiral plane placed at right angles to the thrust, yet every other portion is gradually departing from a right angle, and is exerting its force in an improper direction: thus an arch of this description can never exert its thrust in the direction of the bridge, but is endeavouring to push the abutments obliquely."

"To get the thrust strictly correct I have supposed the arch to be cut into two rings of equal thickness (see fig. 8),

and having considered the external ring as removed, have proceeded to develop the outside surface of the remaining one: this I shall hereafter speak of as the intermediate development, as it is the development of a surface midway between the extrados and soffit or intrados.

"Upon this intermediate development I place the approximate line, and then draw all the courses square to it, by which means we obtain a line in the *centre* of each stone exerting its force in the true direction, as in proportion as the one half of this bed exerts its force in an oblique direction on the one hand, the other half acts in the opposite direction, and is therefore always producing a balance of effect, which resolves the various forces into one exerting all its power in the true direction, which is the object to be obtained."

It will be seen that in all the elevations of oblique arches given by Mr. Nicholson, the bed-joints in the face are *straight lines*, whereas by my rule they are *necessarily curves* (see fig. 5 of the plate illustrating my paper.)

In corroboration of my statement that "many practical men have experienced considerable difficulty in the construction of skew bridges," Mr. Welch has furnished several instances; and the fact which he adduces, that "the executive engineer of a very extensive public railroad has very prudently *applied to a competent person for a definite development of your* (Mr. Nicholson's) *principle*," is really admitting that that principle is not developed with sufficient clearness for practical men in Mr. Nicholson's Treatise. Mr. Welch also seems aware of some latent defect in Mr. Nicholson's rule, for he says, "I am firmly of opinion, after an attentive observation of the *practical working of the best*" (meaning of course Mr. Nicholson's) "*rule*, that it is very injudicious to adopt them," (oblique arches,) "except in cases of absolute necessity;" and so giving it to be understood that oblique arches built on Mr. Nicholson's principle are not so substantial as square ones. Mr. Welch will find it difficult to point out any such want of stability in arches built on my principle.

Every one will be ready to admit that oblique arches should be avoided, from being more expensive, and not equal in appearance; but that they are not inferior from any fault in their "*practical working*," my own practical working has established.

I trust that what has been brought forward shows that Mr. Nicholson's rule and mine are not alike, and therefore that he has no "prior" "claim" to mine; and that my rule possesses advantages not to be found in his.

I am, Gentlemen, your obedient servant,

Park Village East, London, Jan. 19, 1837. CHARLES FOX.

XXXIV. *On the Crystallographical Identity of certain Minerals.* *By* H. J. Brooke, *Esq., F.R.S., &c.**

[With Figures: Plate I.]

I. *Of Zeagonite, Gismondin, Abrazite, Aricite, and Phillipsite.*

THE figures 1 to 4, Plate I., will explain the crystalline relations of these differently named minerals. Fig. 1. is the form of Phillipsite, the crystals being composed of two or four simple crystals arranged in a manner analogous to those of Harmatome; I am not, however, aware that any crystals have yet been found which exhibit this structure as distinctly as in Harmatome.

The line *a b*, fig. 1, is in most crystals slightly raised above the surface, marking the lines of junction of the twin crystals; but in some crystals it is scarcely perceptible, and I have found the inclination of $c' c''$, fig. 2, on crystals with bright faces, differing so little from 90° as to induce my assuming that as the true angle. On one specimen in my possession there are both opake and transparent crystals resembling fig. 1, proving that opacity is not a distinguishing character of this variety of Zeagonite.

Fig. 2 exhibits the first step in the passage of fig. 1 into figs. 3 and 4. I have several instances of two crystals, or bunches of crystals, crossing at right angles and producing the rudiment of the octahedron, fig. 3, the faces of this octahedron being so marked in all the crystals I have seen as to indicate its compound structure. I have observed three minute crystals crossed as in fig. 2, on a specimen in the British Museum explanatory of the crossed octahedron, fig. 4, for a very distinct specimen of which I am indebted to the liberality of Mr. Heuland.

It thus appears that there is no other difference between the crystals of Phillipsite and the other varieties of Zeagonite than in the greater or less complexity of crystalline composition.

II. *Of Murchisonite, Moonstone,* and the *iridescent Felspar from Fredricksvarn, Norway.*

These have the same cleavages, the same angular measurements; and the play of light is observable in the same direction on the particular cleavage plane, by which Mr. Levy distinguished the Devonshire Murchisonite from common felspar.

The three varieties may therefore now be classed together as Murchisonite †.

* Communicated by the Author.

† Mr. Levy's original paper upon Murchisonite will be found in Phil. Mag. and Annals, N.S. vol. i. p. 448.—Edit.

Fig. 1.

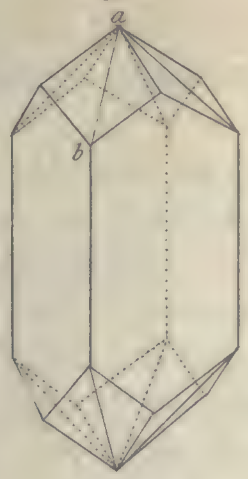


Fig. 2.

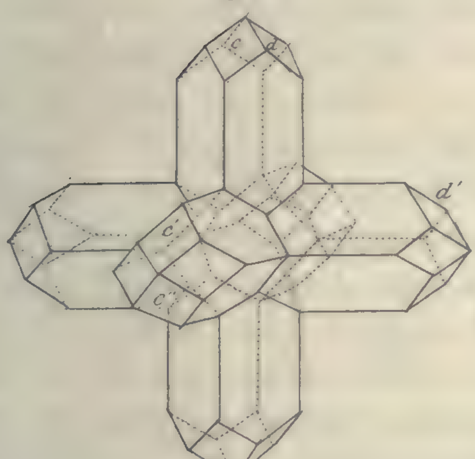


Fig. 3.

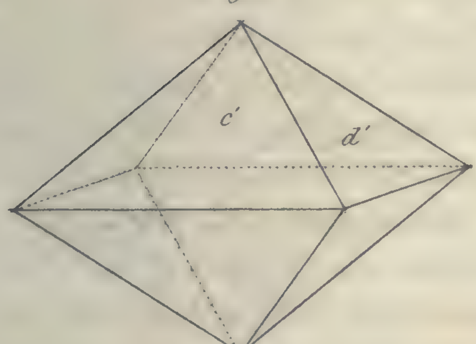


Fig. 4.

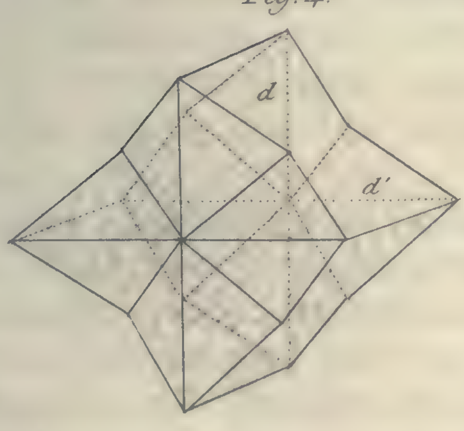


Fig. 5.

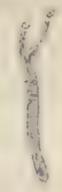


Fig. 6

Syngamus trachealis.

J. Basire sc.

XXXV. *On the Results of Mr. Fox's Experiments on the Production of artificial Crystals by Voltaic Action.*

WE have alluded in page 83 of the last Number of this Journal, in reference to a paper by M. Becquerel in the Third Part of the "Scientific Memoirs," to the communications of Mr. Fox and Mr. Crosse at the Bristol meeting of the British Association relative to the production of artificial crystals by voltaic action. We are now enabled, by the kindness of Mr. Fox in sending specimens of the altered ores for examination, to give a more exact account of the results of his experiments than we could otherwise have done.

Mr. Fox's own statement is as follows: "The experiment referred to was performed in the following manner:—An earthenware trough was divided by a partition of moistened clay into two cells, into one of which was put a piece of the yellow sulphuret of copper, the cell being filled with a solution of the sulphate of copper, and into the other a piece of zinc, and the cell filled with water, either pure or slightly acidulated by sulphuric acid. The zinc was then connected with the copper ore by means of a copper wire passing over the wall of clay. This simple voltaic arrangement soon rendered the surface of the copper ore highly iridescent, then purple, and in the course of a few days gray, the gray crust being covered with metallic copper, deposited in brilliant crystals, and with a slightly greenish soluble salt. This crust resembled gray sulphuret of copper, and increased in thickness after the operation had been continued several weeks."

The crust on the specimens received from Mr. Fox was thin, and the quantity so small that an exact analysis of it could not be made. But the result of the examination to which it was subjected approached so nearly to that of the Cornish sulphuret as to warrant the conclusion of its being the same chemical compound. The soluble salt was found to be a sulphate of copper and iron, which accounts for the iron that the yellow copper ore had lost during its conversion into sulphuret.

Mr. Fox considers that, assuming the gray crust to be sulphuret of copper, as it turns out to be, these results explain why metallic copper occurs in our mines in contact with gray and black copper ore, and not with the yellow sulphuret of that metal; and likewise why the former is generally found nearer the surface than the latter, and also near cross courses and in situations where it is most exposed to the action of

water, the expelled ferruginous matter being indicated by the "*gossan.*" This usually consists of quartz, as well as iron-ochre, &c., and it abounds in copper veins, but not in those of tin.

XXXVI. *Remarks on* Faraday's *Hypothesis with regard to the Causes of the Neutrality of Iron in Nitric Acid. By Professor* SCHŒNBEIN.*

IN the July Number of the Philosophical Magazine for 1836, Mr. Faraday has set forth an equally simple and ingenious hypothesis on the cause of the passive condition assumed by iron in common nitric acid under certain circumstances. This distinguished philosopher supposes that the peculiar action of this metal is caused, first, by a thin layer of oxide which is formed round the iron wire under these circumstances; and, secondly, by the property of this oxide to be insoluble in nitric acid of a certain degree of concentration. According to Faraday, therefore, the real cause of the inactivity of the iron would be purely mechanical; that is, the inactivity of the iron would originate in the fact, that the metallic iron and nitric acid do not come into actual contact. In the same way Faraday seems also to explain the fact which I have observed, that when a complete circuit of the electric current is formed, oxygen gas is evolved at the positive iron wire. As probably his paper on the subject in question will find a place in this [Poggendorff's] journal, I do not consider it as necessary to enter into a more detailed account of the hypothesis of which we are speaking, for the purpose of subsequently referring to it, and I proceed, therefore, at once to bring forward the facts which appear to stand in opposition to those of Faraday. I must first remark, that the surface of an iron wire which has been made passive by repeated immersions in nitric acid of 1·35 (see my last paper†) exhibits a much cleaner and brighter metallic surface than a wire which has been cleaned in any other manner, and therefore not the slightest trace of a coating of oxide can be observed by the eye. I will, however, not lay much stress upon this circumstance, although I think it deserves some consideration. In one of my former treatises I have mentioned the fact, that iron wire, in whatever way it has been made passive towards common nitric acid, acts like common iron in considerably diluted acid, whilst an iron wire

* From Poggendorff's *Annalen der Physik und Chemie*, vol. xxxix. p. 137.

† Prof. Schœnbein's paper here referred to appears to correspond with his first paper on the subject in Lond. and Edinb. Phil. Mag. vol. ix. p. 53. —EDIT.

acting as the positive pole, shows the most absolute chemical indifference to nitric acid of all strengths. This fact appears to me to speak strongly against the truth of the English philosopher's hypothesis; for if we assume for an instant that at the moment of immersion of the positive iron wire in the diluted nitric acid, (in consequence of the decomposition of water thus caused,) a thin layer of this questionable oxide is formed upon it, and that in this circumstance the evolution of the oxygen gas originates, we cannot conceive how the oxide so formed can remain for one instant in dilute acid without being dissolved in it, viz. in an acid so diluted that, according to Faraday, the oxide cannot remain any longer indifferent. In other words, if the chemical indifference of the iron to the nitric acid were materially influenced by the degree of dilution of the latter, the iron under the above-mentioned circumstances would remain active, a nitrate of iron would be formed, and no evolution of oxygen from the metal would take place. Experience, however, shows that exactly the reverse of what one might have expected, according to the above-mentioned hypothesis, takes place. Faraday, to be sure, mentions that iron in nitric acid (the strength of which, however, is not given,) is dissolved even when it acts as the positive pole in it. According to my experiments, of which I may assert that they were conducted with the greatest possible care and accuracy, no trace of this metal is dissolved under the circumstances stated in nitric acid which is several times diluted with water. I kept an iron wire, which was united to the positive pole of a *couronne des tasses* of 15 jars, for many hours in such an acid without being afterwards able to distinguish the smallest trace of iron in it. If however to such an experiment acid of common strength, as, for instance, of 1·35, is added, the result is somewhat different; in this case it contains, after some time, always a little oxide of iron. According to my conviction this is not formed in the acid, but a nitrate of iron is produced on that part of the wire which reaches above the acid, by the acid vapours continually rising, which nitrate is then conducted down into the acid by capillary action. A more important fact, to which I must draw attention, is the circumstance that the iron wire which dips into the diluted acid and is indifferent to it, is acted upon as soon as the electric current ceases to pass through it. If, for example, we allow the iron wire to remain in the acid to be examined, and interrupt the current anywhere, the wire immediately appears surrounded by heavy yellowish brown striæ, which are a nitrate of iron. From these facts it ap-

pears, that the most important cause of the chemical indifference of the iron to the nitric acid is neither owing to its being surrounded by a thin pellicle of oxide, nor to a certain quantity of water in the acid, but directly to the electrical current in whatever manner it acts. Moreover, it is clear, that if the indifference of the positive iron wire depended upon a thin coat of oxide which surrounds it, the same wire ought to remain passive when it is separated from the pole, and put into common nitric acid, which however is not the case. The fact that the positive iron wire is acted upon similarly in other dilute acids as in nitric acid is rather unfavourable to Faraday's hypothesis. It is well known that iron becomes quite passive by being merely once dipped into fuming nitric acid: how then is the pellicle formed in this case? I suppose only by the decomposition of the nitric acid, because nothing else is possible. I doubt however very strongly whether this takes place; but if it does not take place, it is very difficult to ascertain in what manner the iron does become oxidized. I must, however, remark that the galvanometer during the dipping of the iron in highly concentrated nitric acid indicates a weak electric current, but it appears to me that this does not prove the oxidation of the metal. To the above remarks I must add one more, which I think is not unimportant as regards this subject. In my last paper I spoke of an action of an acid of 1·35 upon the iron which took place by starts, and I showed that it was occasioned by the metal becoming active and passive. Faraday would explain this appearance by the supposition that at one instant a pellicle of oxide is formed round the wire which protects it from the action of the nitric acid, but that in the next the pellicle is dissolved in the acid, and would expose by that means the clean metallic surface of the wire to the acid fluid. But this manner of explaining it contains a contradiction in itself; because it first makes out the pellicle to be insoluble in nitric acid, and then to be soluble; it cannot therefore be correct. Moreover, one might put the unanswerable question, why iron by frequent immersions in common nitric acid is rendered passive; therefore, why an oxide is formed which in the first place enters into combination with the acid, but afterwards remains in the same acid an insoluble oxide? All the reasons above given decide me to suppose that Faraday's views concerning the passive state of the iron do not explain it satisfactorily.

Bâle, October 2, 1836.

Note from Professor Faraday *to Mr. Richard Taylor on the preceding Paper.*

Dear Sir,

I AM much obliged to you for a sight of M. Schœnbein's paper, the experiments and observations in which are excellent. The cause of the phænomena he has so well distinguished is indeed exceedingly difficult to be distinguished at present, and I was in hopes that the doubt on my mind when I ventured the view referred to would be evident from my words, "My strong impression is," &c.* Moreover, M. Schœnbein, and also M. Alb. Mousson in an attempt which he has made to explain the cause†, have not given my view clearly. I have said, that my impression is, that the surface of the metal is oxidized, or else *that the superficial particles of the metal are in such relation to the oxygen of the electrolyte as to be equivalent to an oxidation* ‡; meaning by that, not an actual oxidation, but a relation like that of the particles of amalgamated zinc to the oxygen of the water in dilute sulphuric acid *before* the electric current which tends to be formed is established. (See Experimental Researches, Eighth Series, par. 949.)

The state seems to me to be one of a very delicate equilibrium of forces, though of course that condition of things which can produce it can also retain it; and this notion seems to be confirmed by the intermitting action originally mentioned by Herschel. I quite agree with M. Schœnbein's conclusion, "that Faraday's views concerning the passive state of the iron do not explain it satisfactorily," and I only regret that the other views which have been put forth are, as far as I can perceive, not more satisfactory. For instance, that by M. Mousson leaves out of view the capital and leading fact, that a piece of iron in the peculiar state may remain for six months (as I know by experiment) in nitric acid of sp. gr. 1·35 without being in the least affected, although no platina or primary metal is in contact with it.

I was somewhat struck the other day, whilst reading in your useful 'Scientific Memoirs' M. Nobili's paper on a new chromatic scale, on finding that he accounts for the colours of his thin plates by assuming films of oxygen and acid ‖; which he considers as adhering permanently to the surfaces of the platina, iron, and steel plates which he used in his experiments, without

* Lond. and Edinb. Phil. Mag., vol. ix. July 1836, p. 61.

† *Bibliothèque Universelle de Genève*, Sept. 1836, p. 165.

‡ Lond. and Edinb. Phil. Mag., vol. ix. July 1836, p. 61.

‖ Scient. Mem., Part I. p. 108.

however being in combination, chemically, with the metal. This is going much further in the idea of association without combination than I have done. I merely mention the fact for the purpose of directing M. Schœnbein's attention to the condition of the iron and steel plates in Nobili's experiments. My own impression at present is, that Nobili's thin plates consisted of peroxide of lead formed at the positive electrode in the solution of acetate of lead used; and that it is just possible some of the differences between his tints and those of Newton may depend upon, and some of his conclusions be affected by, the circumstance that peroxide of lead has a powerful specific action on light, and is even in moderately thin plates deeply coloured.

I trust that M. Schœnbein's perseverance will very shortly produce that reward it so well merits, the true explication of the cause of the peculiar state of iron.

I am, my dear Sir, very truly yours,

Royal Institution, Jan. 21, 1837. M. FARADAY.

XXXVII. *Report by a Committee of the Royal Society (of Edinburgh) regarding the New Dioptric Light of the Isle of May.**

THE Committee of the Royal Society appointed to cooperate with the Commissioners for Northern Lights, met at Dunbar, on Wednesday, 26th October 1836. Present, Right Hon. Lord Greenock, V.P.R.S.E.; Mr. Robison, Sec. R.S.E.; Dr. Traill, Dr. Christison, Professor Forbes. Present also, Mr. Alan Stevenson, on the part of the Commissioners.

The Committee were requested to compare the new fixed dioptric light on the Isle of May, thirteen miles distant, with the old catoptric light, placed on a temporary erection near it. From the first point of observation, near the town of Dunbar, the Committee were unanimous in pronouncing the great superiority of effect of the new light, which appeared brighter than the old one in the ratio of not less than 4 or 5 to 1. But upon changing the position of the point of observation to the eastward, the difference became less marked, and at a distance (from the first station) of about one and a half miles was scarcely appreciable, though the new light appeared whiter and somewhat better defined. The reason of this prodigious inequality of the old light in different azimuths,

* Communicated to the Committee of Northern Lights, 28th October 1826, and to the Lond. and Edinb. Phil. Mag., by Prof. Forbes.

Mr. Alan Stevenson, the assistant-engineer, explained to be, that since the divergence of the reflected beam is produced by placing the lamp a little out of the focus of the mirror, the central pencil of reflected light is much less scattered than the lateral pencils, which in an azimuth [azimuths] exactly intermediate between the axes of two adjacent reflectors is [are] very feeble indeed. The evening of the 26th was neither remarkably clear, nor remarkably the reverse, and the members of the Committee were satisfied that a slight increase of haze would have rendered the old light wholly invisible *at* the town of Dunbar, which appears to be placed (in the present temporary position of the reflecting system) in the line of least illumination.

The number of mirrors in the old system being twenty-four*, and the divergence given to the reflected light 15°, a certain portion of light is thrown all round the horizon, but the intensity in different directions varies (as has been said) prodigiously. The divergence of 15° in a vertical plane also taking place as a necessary consequence of this method of expanding the reflected beam, by unfocusing the lamp, produces a useless and most injurious expenditure of light in directions in which it can never be seen. On the other hand, the dioptric system of hoops, besides the great advantage obtained by substituting refraction for reflection (from the less loss of light) rigorously fulfils the requisite geometrical condition of spreading a plane of light, exhausting the whole available light of the lamp, of precisely equal intensity in all directions. This has never been attempted by any combination of reflectors.

The distance of the Isle of May from Dunbar being thirteen miles, the range of one mirror on the old system extending through about 15°, would not differ much from a linear distance of three miles in a direction perpendicular to a line joining Dunbar and the May. The space between maximum and minimum light would therefore be about a mile and a half. The space walked over must have nearly included a complete series of the variations of the light. Assuming the average light of the mirrors at half that of the dioptric light (which certainly does not seem too much), we have a superiority of two to one in favour of the latter at a distance of thirteen miles. These are to be considered, however, but as rude ocular estimations.

* It appears that at the Isle of May there are only twenty-two reflectors of a larger size, the divergence being proportionally increased in order to complete the circumference. (Initialed) J. D. F.

But it is not to be inferred that the ratio of the intensities of the two systems will remain the same at all distances. Were the refracted light in one plane (instead of having a vertical divergence of 6°), and were the reflected light comprised within a cone uniformly illuminated (which is not the case), the light from the first would vary inversely as the distance, from the second inversely as the square of the sum of the distance and a constant which depends upon the divergence; but neither of these conditions being fulfilled, the respective illuminations must vary with the distance according to two other distinct laws, each much more complicated. It is clear, therefore, that the comparison made at one given distance will not apply to any other given distance. But still the effect of distance in modifying intensity, may be much more accurately estimated in the case of the prismatic than of the reflecting arrangement.

The following conclusions seem to be warranted:

1. That, at a distance of thirteen miles, the mean effect of the new light is very much superior to the mean effect of the old light (perhaps in the ratio of two to one).

2. That at *all* distances the new light has a prodigious superiority to the old, from the equality of its effects in all azimuths.

3. That the new light fulfils rigorously the conditions required for the distribution of light to the greatest advantage.

4. That at distances much exceeding thirteen miles, the new light must still be a very effective one, though to what extent the Committee have not observed. The light is understood to be still a good one, when seen from Edinburgh at a distance of about thirty miles.

XXXVIII. *Remarks on* Mr. Brett's *Experiments on the Solubility of Metallic Oxides and Salts in Muriate and Nitrate of Ammonia. By* L. Thompson, *Esq., Member of the Royal College of Surgeons.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I OBSERVE a serious omission in the laborious experiments of your correspondent, Mr. Brett, on the solubility of metallic oxides and salts in muriate and nitrate of ammonia, inserted in your last Number, p. 95, which, if uncorrected, is likely to produce an erroneous opinion as to the

cause by which such solution is effected. From the general tenor of the paper it is evident that Mr. Brett supposes, that when a metallic oxide or salt is dissolved in a solution of muriate or nitrate of ammonia, no decomposition takes place, and that the metallic oxide or salt is merely, as he expresses it, "dissolved by the muriate or nitrate of ammonia." This, however, is by no means a correct view of the case; for on repeating a few of the experiments proposed by Mr. Brett, I find that when a solution of muriate of ammonia is boiled with any of the under-mentioned metallic oxides or salts, viz.

Protoxide of mercury, peroxide of mercury, red oxide of mercury by nitric acid, protoxide of lead, protocarbonate of lead, oxide of zinc, carbonate of zinc, peroxide of copper, and subnitrate of bismuth,

a very free evolution of ammonia takes place, and a metallic chloride is formed, which combines, in some instances, with part of the muriate of ammonia, forming a triple salt. The theory of this process is too simple to require an explanation.

I have not performed any other of Mr. Brett's experiments, but deem the few I have thus briefly detailed, sufficient data from which to infer that decomposition occurs in almost every instance; for although it may be difficult to prove that phosphate of ammonia and muriate of lime are formed when phosphate of lime is dissolved in muriate of ammonia, yet analogy favours such an opinion. The inefficacy of nitrate of ammonia when contrasted with the muriate may be ascribed to the trifling affinity existing between nitric acid and the metallic oxides when compared with the attraction of their metals for chlorine.

I have no doubt that on a repetition of his experiments Mr. Brett will find ample reasons for changing his present idea, for when large quantities, as two drachms, are operated on at once, no better test than the nose is requisite for determining the presence of the ammonia evolved.

I am, Gentlemen, yours, &c.

Roebuck Place, Great Dover Road, Feb. 6, 1837. — LEWIS THOMPSON, M.R.C.S.

Errata in Vol. ix.—In the heading of my paper, p. 442, for "*Iodous Acid*," read "Iodic Acid;" and in the same page, line 19, for "$2\frac{1}{2}$ *atoms or 295 grains of recently precipitated oxide of silver*," read "5 atoms or 590 grains", &c.

Dec. 20, 1836.

XXXIX. *On the Solar Eclipse of May* 15*th*, 1836. *By* C. Rümker, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the calculation of this eclipse, the following method has been adopted: Suppose ϕ to denote the geocentric latitude, τ the apparent time from noon, δ the north polar distance of the sun, π the difference of horizontal parallaxes reduced for the spheroidical figure of the earth, λ parallax in longitude, β moon's apparent latitude — sun's latitude.

$$\mathrm{H} = \frac{3600''}{☾\text{'s relat. hor. mot. in Æ}}.$$

μ for mean $\left\{\begin{array}{l}\text{Noon in Green. 15 May } 75^\circ\ 53'\ 40''\!\cdot\!6 \\ \text{Midnight} \quad\ \text{15 May } 76 \quad\ 3\ \ 21\ \cdot\!0\end{array}\right.$ $\begin{array}{l}\mathrm{R}\ \text{sun's} \\ \varrho\ \text{moon's}\end{array}\Big\}$ semidiameter.

$\mathrm{S} = \mathrm{R} - r + \varrho$, $\mathrm{D} = \mathrm{R} - r - \varrho$, where r is to be taken out of the following table to be entered with h cotan ϕ cos τ = tan m.,

$$\cos\phi \sin\tau = \sin n, \quad \frac{\sin(\delta - m)}{\tan n} = \cot r, \quad \frac{\sin n}{\sin r} = \cos h$$

$$\pi \cos h \cos(\mu \pm \nu) = \lambda, \quad \pi \cos h \sin(\mu \pm \nu) = \text{parallax in lat.}$$

$$(\sqrt{(\mathrm{S}^2 - \beta^2)} \mp \lambda)\, \mathrm{H} = d\tau.$$

The upper sign of ν is to be used in the forenoon, the lower in the afternoon. When the sun is in the descending signs $180^\circ - \mu$ is to be used in lieu of μ.

Table of Diminution of the Sun's Semidiameter.

h°	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33
r″	1·303	1·564	1·823	2·082	2·340	2·598	2·854	3·110	3·365	3·619	3·872	4·123	4·374	4·623	4·870	5·116	5·361	5·604	5·845	6·085	6·322	6·558	6·792	7·023	7·253	7·480	7·705	7·928	8·148

h°	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62
r″	8·365	8·581	8·793	9·003	9·210	9·415	9·617	9·814	10·010	10·202	10·392	10·578	10·761	10·941	11·118	11·290	11·460	11·626	11·789	11·948	12·103	12·254	12·402	12·547	12·687	12·823	12·956	13·084	13·209

I measured at Hamburgh at the time of the greatest obscuration, the distance between the sun's and moon's south limbs, 1′ 57″·25, whence the apparent distance of centres is found 63″·9, and the apparent difference of latitude 64″·0, instead of which the Nautical Almanac gives 70″·6. Hence it follows that a correction of −6″·6 must be applied to the latitude

given in the Nautical Almanac, which is further corroborated by the observations made at Copenhagen, Kœnigsberg, &c.

In comparing the observations made at places situated on the borders of the Annulus, with the computation according to the elements of the Nautical Almanac, I find a better accord by assuming for latitude correction $-7''\cdot63$, and for correction of the sun's semidiameter $-1''\cdot5$, and for correction of the moon's semidiameter $+0''\cdot5$.

These corrections have been previously applied in the following calculations. B means beginning; BA, beginning of annulus; EA, end of annulus; E, end; d. (☉ ± ☾), difference of sum or difference of semidiameters; d β, of latitude; d π, of parallax.

Places and Observers.		Mean Time of Observation.	Ecliptic Conjunction.	d (☉ ± ☾)	d β	d π
		h m s				
Altona. Schumacher.	B	2 43 50·75	2 46 51·02	+2·1776	+0·1137	+1·1131
	E	5 21 23·15	2 46 52·3	−2·1810	+0·1523	+1·5385
Apenrade. Hanssen.	B	2 40 36·8	2 45 14·37	+2·1842	+0·2045	+1·0263
	BA	4 0 4·8	2 44 52·2	+2·2260	+0·4730	+1·1672
	EA	4 4 23·8	2 44 52·8	−2·1834	−0·1912	+1·5387
Berlin. Encke.	B	3 2 43·8	3 0 41·3	+2·1751	+0·1427	+1·2394
	E	5 37 31·9	3 0 45·1	−2·1820	+0·1670	+1·5782
Bern. Treschel.	B	2 37 8·6	2 37 17·63	+2·1962	−0·2997	+1·3277
	E	5 16 48·26	2 36 43·36	−2·2468	+0·5608	+1·5060
Bremen. Cluver.	B	2 38 7·0	2 42 13·38	+2·1760	+0·0787	+1·1087
	E	5 16 56·9	2 42 14·8	−2·1850	+0·1998	+0·8576
Bremerhaven. Thulesius.	B	2 37 27·0	2 41 58·7	+2·1783	+0·1264	+1·0751
	E	5 15 27·0	2 41 24·17	−2·1821	+0·1678	+1·5286
Brussels. Quetelet.	B	2 16 0·5	2 24 35·66	+2·1750	−0·0300	+1·0779
	E	4 59 47·3	2 24 33·9	−2·2058	+0·3635	+1·4712
Copenhagen. Petersen.	B	2 55 52·8	2 57 28·93	+2·1876	+0·2378	+1·0605
	BA	4 15 53·2	2 57 13·8	......	......	+1·4532
	E	5 29 32·9	2 57 11·0	−2·1757	+0·0121	+1·5802
Gera. Engelhard and Metz.	E	5 33 43	2 55 23·9	−2·1926	+0·2724	+1·5600
Greenwich. Airy.	E	4 39 12·32	2 7 4·62	−2·2067	+0·3693	+1·4070
Halifax. Waterhouse.	E	4 27 7·0	1 58 43·9	−2·1914	+0·2624	+1·384
Hamburg. Peters.	B	2 44 7·4	2 47 5·7	+2·1776	+0·1135	+1·1101
	E	5 21 30·5	2 46 59·5	−2·1810	+0·1526	+1·5442
Hamburg. Rumker.	B	2 44 2·2	2 47 0·54	+2·1776	+0·1135	+1·1101
	E	5 21 40·5	2 47 8·89	−2·1810	+0·1526	+1·5390

Places and Observers.		Mean Time of Observation.	Ecliptic Conjunction.	d (☉ ± ☾) d β d ϖ
		h m s		
Hanover. Lahmeyer.	B	2 43 49·04	2 46 6·69	+2·2139+0·4152+0·9811
	E	5 21 48·73	2 45 56·43	+2·1866+0·2182+1·5304
Jena. Schrœn.	E	5 31 35·0	2 53 28·45	−2·1942+0·2845+1·5501
London, Fleet-street W. Simms.	B	1 51 13·0	2 6 59·43	+2·1750+0·0430+0·9154
	E	4 38 47·0	2 6 47·2	−2·2066+0·3685+1·4115
Louvain. Crahay.	B	2 17 37·3	2 25 47·55	+2·1748−0·0289+0·8620
	E	5 0 52·6	2 25 33·6	−2·2049+0·3580+1·4748
Makerston. Sir Thomas Brisbane.	B	1 36 51·2	1 57 17·2	+2·1967+0·3111+0·6287
	BA	3 1 4·2	1 57 19·1	+2·2710+0·6532+0·8621
	EA	3 5 11·6	1 57 5·5	−2·1870−0·2284+1·7380
	E	4 23 0·6	1 57 4·0	−2·1809+0·1515+1·3888
Neumühfen. Zahrtmann.	B	2 43 54·4	2 46 54·4	+2·1775+0·1128+1·1103
	E	5 21 20·6	2 46 49·9	−2·1810+0·1530+1·5475
Neu Strelitz. Lorenz and Becker.	B	3 0 28·0	2 59 31·5	+2·1764+0·0879+1·1987
	E	3 54 58·	2 59 17·7	−2·1799+0·1367+1·5722
Rostock. Karsten.	B	2 54 43·1	2 55 31·8	+2·1788+0·1349+1·1418
	BA	4 14 19·7	2 55 39·6	+2·4635−1·1568+2·1523
	EA	4 17 58·2	2 55 35·3	−2·6073+1·4378+0·7263
	E	5 29 58·2	2 55 28·0	−2·1401+0·0997+1·5704
Shooter's Hill. Simms and Gilby.	B	1 51 52·1	2 7 26·46	+2·1750+0·0402+0·9229
	E	4 39 20·1	2 7 26·7	−2·2080+0·3768+1·4150
Stettin. Dancke.	B	3 7 51·7	3 5 21·0	+2·1782+0·1256+1·2064
	E	5 41 16·3	3 5 21·9	−2·1782+0·1061+1·5925
Stralsund. Steinort.	B	2 59 44·2	3 0 3·34	+2·1803−0·1576+1·3025
	BA	4 18 7·0	2 59 24·9	+2·1891−0·2469+1·6207
	EA	4 22 26·6	2 59 28·2	−2·2254+0·4705+1·2856
	E	5 33 49·2	2 59 25·8	−2·1785+0·1111+1·5605
Strassburg. Herrens-scheider.	B	2 36 25·14	2 38 1·37	+2·1830−0·1911+1·2692
	E	5 16 44·95	2 37 48·6	−2·2232+0·4575+1·5100
Tondern. Petersen.	B	2 37 15·1	2 42 34·9	+2·1839+0·2009+1·0156
	BA	3 57 26·88	2 42 34·68	+2·1793+0·1357+1·2584
	EA	4 1 48·1	2 42 34·2	−2·1797+0·1421+1·3404
	E	5 14 51·12	2 42 31·3	−2·1775+0·0903+1·4744
Wurzburg. Schrœn.	B	2 47 4·0	2 46 54·4	+2·1773−0·1071+1·2704
Vienna. Littrow and Hallasckka.	E	5 34 37·1	3 12 31·85	−2·2050+0·3590+1·5910
Zeitz.	E	5 32 40	2 54 37·6	−2·1911+0·2602+1·5598

N.B. Should it be in your power to procure me any observations made of this eclipse in England or America, you would greatly oblige thereby

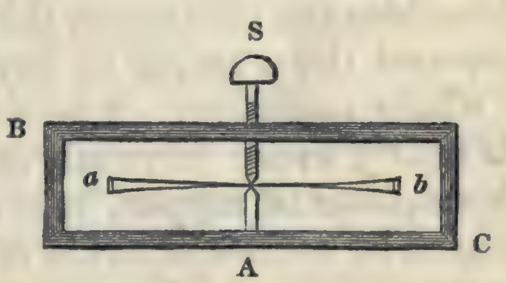
Your obedient Servant,

Hamburg, Oct. 5, 1836. C. RÜMKER.

XL. *A simple Mode of exhibiting Newton's Rings, and a Mode of exhibiting the Fixed Lines in the Spectrum. By the* Rev. W. RITCHIE, *LL.D., F.R.S., Professor of Natural Philosophy in University College, and in the Royal Institution of Great Britain.**

1. TO exhibit Newton's rings a lens of a long focal distance is generally considered necessary, which is both expensive and difficult to obtain. In a lecture which I delivered nearly two years ago in the Royal Institution, I showed a very simple mode of performing the experiment, which, I have no doubt, will be acceptable to many of your readers. Take two circular pieces of thin plate glass (Dutch plate) about six or eight inches diameter.

Gild a ring of one of the plates about a quarter of an inch broad from the circumference of the circle with gold-leaf, place the plates over each other, and by means of a rectangular frame of iron or brass and a screw, bring the plates to touch in the centre. Let *a b* represent the glass plates, B C the rectangular frame, S a screw, and A a projecting point.



By means of the screw the plates will be brought to touch in the middle, whilst they are separated at the circumference by a single gold-leaf. When this is held so that light from the sky or a lamp falls obliquely on the plates so as to be reflected by the under plate to the eye, the rings will present themselves in circles round the dark spot in the centre.

2. Procure a prism of good flint glass, having one of its angles containing 70 or 80 degrees. Place two thin slips of metal with smooth edges in an opening in a window-shutter, through which the white light of the clouds is admitted. View this *film* of light through the *large* angle of the prism kept close to the eye, and the principal fixed lines as well as many of the others will be distinctly visible. If a bottle containing nitrous gas be placed opposite the opening, the lines will become more strongly marked and more numerous. With one

* Communicated by the Author.

of my finest prisms this spectrum appears like a piece of *striped* cloth.

XLI. *On a Method of producing Achromatic Light in Solar and Oxy-hydrogen Microscopes, and on the Effect of a Current of Air upon the Rays that occasion Heat.* By the Rev. J. B. Reade, *M.A.**

IT will no doubt be admitted, that the experiments of Melloni on radiant heat and light have not only given us much insight into the nature of these two agents, but have also tended to solve the important problem which had been raised as to their identity. Henceforward, therefore, any new facts can occupy but a secondary place, and, however interesting in themselves, their real value must depend on their conforming to a theory which has already been independently proved.

The received theory is, that the luminous and calorific rays are two essentially distinct modifications which the æthereal fluid suffers in its mode of existence, and that they may be easily separated the one from the other by transmitting the æthereal fluid through screens of different substances.

Melloni, in his experiments, has employed a variety of radiating sources, and received the rays on screens of coloured and uncoloured glass, liquids, and crystallized bodies. The tables with which he has furnished us of both solid and liquid bodies, exhibit the common thickness of the screens employed, and besides the substance, the indications of the thermomultiplier, and the number of rays transmitted as compared with the whole radiation. The effect produced appears to vary with nearly every variation of the substance, and it is only with sulphate of copper, and a peculiar species of green glass coloured by means of oxide of copper, that no calorific action is perceptible.

In following out Melloni's idea of the separation of the calorific and colorific rays†, I have sought in my own experiments to attain this object by modes sufficiently effective in themselves, and, at the same time, admitting of such easy application to the solar and oxy-hydrogen microscopes, that achromatic object-glasses, and objects mounted in balsam, may be used without risk or danger.

That method which I have found to be very successful, and attended with the least possible amount of trouble, consists, as I have stated in a paper communicated to the Royal Society, in a certain position of the condensing lens and the field-glass of the solar microscope, by which a difference of at least 50°

* Communicated by the Author.

[† A translation of Melloni's paper on this subject will be found in Scientific Memoirs, Part III. p. 388.—Edit.]

of temperature is indicated by the thermometer at the foci of these two lenses. It appears, however, upon trial, that the crossing of the rays between the lenses so materially affects the illuminating power, *when a terrestrial source of light is used*, that the applicability of this arrangement to the oxy-hydrogen microscope becomes extremely questionable. I have therefore proposed two new methods, which appear both to improve the light and to remove all injurious heat.

The light is improved by a combination of different lenses. After the rays proceeding from a column of lime have been rendered parallel, they are received on what may be properly termed the condensing lens of the instrument. A very deep *double concave lens* is then placed within the focus now obtained, so that the condensed emergent pencil is of small diameter, parallel, and nearly achromatic. We then proceed as usual with the field-glass and the object-glass. With this arrangement only have I been able to see, with entire satisfaction, the longitudinal and cross lines on those scales of butterflies' wings which are received as *test objects*; and this distinctness evidently arises from effecting, by means of the concave lens, an almost entire removal of the blue rays, which in any other adjustment of lenses occupy the middle of the illuminated disc.

The heat is also partially removed by this combination of lenses; for, as calorific rays, like luminous rays, are susceptible of refraction, it will follow from the different positions of the principal foci of light and heat in the axis of the condensing lens, that when the concave lens renders the colorific rays parallel, the calorific rays will, in this case, diverge. But to procure a practically efficient removal of the heat, I have proposed to transmit the rays through a *current of air*. That such a method should not have occurred to Melloni cannot but be a matter of surprise, though perhaps, indeed, it may seem somewhat unphilosophical to blow with a pair of common bellows upon the rays of the sun, under the expectation of putting out the heat. But, be this as it may, the effect is most decisive; for, as it will presently appear, this useful household instrument, like the traveller in the fable, *blows both hot and cold.*

After the idea of using the bellows had suggested itself to my mind, I took the first opportunity which occurred of placing a delicate thermometer in the focus of the condensing lens of my solar microscope. By directing a current of air upon it, the calorific energy of the solar beam was suddenly diminished, and, after a few blasts, the thermometer indicated about 60° of temperature only. It is quite evident that no-

thing would be easier than to direct a continued current upon a thin screen of glass placed just behind the object in the microscope, by which means the uniform and low temperature thus steadily maintained would ensure perfect safety even for the most delicate animalcules.

Under the impression that some change would be indicated by the thermometer if a current of air were impinged upon it when it indicated the ordinary atmospheric temperature, I directed against the bulb about thirty blasts with the bellows. I confess I was not prepared to find, after just witnessing the rapid fall of the mercury, that a decidedly opposite effect would be produced. The mercury, however, now rose in different thermometers, placed both in the house and also in the open air, between five and seven degrees. In each experiment the bellows, before they were used, had acquired the temperature of the surrounding air, and the first four or five blasts produced at least one third of the whole elevation. We have, therefore, before us these two facts, that a current of common air is a powerful absorbent of the rays that occasion heat; and that in the blast of bellows by which this current is maintained there are equal and opposite forces tending to produce equilibrium. These two forces consist in a refrigerating and a calorific current, which have each a separate existence. The former is effective if the mercury be artificially raised, and the latter if it indicate the temperature of the air. In the first case the calorific principle in the solar ray is imparted to the current of air and absorbed; and in the second, the calorific principle in the current of air is imparted to the thermometer and retained. And hence it must be admitted, however it may appear to contradict our sensations, that the blast of a pair of bellows is at least 5° warmer than the air they receive. To account for this fact it is only necessary to refer to the general law established by Clapeyron, whose memoir on the motive power of heat has found a place in Part I. of Mr. Taylor's new and very valuable quarterly publication entitled "Scientific Memoirs." The law at which Clapeyron has arrived, and which is applicable to all the substances of nature, solid, liquid *, or gaseous, is, that if the pressure supported by different bodies, taken at the same temperature, be augmented by a small quantity, quantities of heat will be disengaged from them, which will be proportional to their dilatability by heat. But it is beside my purpose to enter further into this subject. I have already accomplished the object I had in view by showing that the free passage of a current of air absorbs the calo-

* The impact of a current of water on a thermometer produces an effect similar to the impact of a current of air.

rific rays, and that the introduction of a concave lens tends to achromatize and improve the light. Our solar and oxy-hydrogen microscopes, therefore, instead of being used for purposes of amusement only, and limited to the exhibition of objects which are not affected easily by heat, may henceforward be employed for purposes of scientific investigation, and thereby assume the more important rank of valuable philosophical instruments.

Peckham, Feb. 10, 1837.

XLII. *On Professor* Müller's *Account of the Reflex Function of the Spinal Marrow. Communicated by* Marshall Hall, *M.D., F.R.S., &c.*

[Continued from p. 129, and concluded.]

"DR. MARSHALL HALL distinguishes four kinds of muscular contraction: 1st, The voluntary, which appears to depend on the brain; 2nd, The respiratory*, which appears to depend on the medulla oblongata; 3rd, The involuntary, which depends on the nerves and muscles, and requires the immediate application of stimuli to the muscles provided with nerves, or to their nerves; and 4th, The reflecting, which continues, in part, after the voluntary and respiratory have ceased, and is connected with the medulla spinalis. It ceases after removal of the spinal marrow, though irritability does not diminish. In this fourth the motor stimulus does not originate in a central part of the nervous system, but at some distance from the centre; it is neither voluntary nor direct in its course, but rather excited by peculiar stimuli, which act, not immediately on the muscular fibres and motor nerves, but on membranous expansions, from which the stimulus is conducted to the medulla spinalis. Dr. Marshall Hall illustrates the importance of this reflecting function of the medulla oblongata and spinal marrow by some instances. The prehension of food is a voluntary act, and cannot be performed after removal of the brain; the passage of the morsels of food over the glottis and through the pharynx depends on the reflex function, and still continues after the brain is removed. Although, for instance, the muscles which are active in this case, may also act voluntarily, yet the presence of the morsel in the pharynx produces a series of violent motions, which have been described above (p. 479.), and which arise from the stimulus of the morsel

* I am now of opinion that respiration itself is a part of the reflex or excito-motory function, and dependent upon appropriate excitor nerves.—M. H.

acting on the sensitive mucous membrane, and this perception exciting the medulla oblongata to discharge in the motor nerves. Dr. Marshall Hall regards the further act of deglutition in the œsophagus as the effect of the stimulus acting immediately on the muscular fibres of the œsophagus, and the result of the irritability of the latter, which may appear very doubtful*. Even in beheaded young animals we may, however, as already shown, observe the reflected motorial excitement still following mechanical stimulation of the pharynx. Dr. Marshall Hall next shows the permanent influence of this function in the sphincters. The sphincter ani remains closed in a tortoise after decapitation, so long as the lower part of the spinal marrow is uninjured, but instantly becomes flaccid and opens, when the spinal marrow is removed.

"Dr. Marshall Hall divided the spinal marrow in a live *Coluber natrix* between the 2nd and 3rd vertebræ. The motions ceased at once, and when the animal was not stimulated it remained quiet. But if it were stimulated, it continued moving for a long time; for at every altering position new parts of its surface came in contact with the ground: gradually it again became quiet, but the slightest touch again renewed the motion.

"Dr. Marshall Hall shows very beautifully the relation of the voluntary, respiratory, and reflected motions, when he endeavours to prove, that the reflected motions which take place after loss of the brain are not dependent on true *sensation*, but only on the centripetal nervous actions which take place in sensations. Sensation, will, motion, are the three links of the chain, when a motion is induced by pain; but if the middle link be destroyed, the connexion between the first and second with the consciousness ceases. We believe also that the reflected motions on stimuli of the skin, which take place after the removal of the brain, do not contain any proof that the stimulus excited true sensation in the spinal marrow; it is rather the centripetal conduction of the nervous principle which commonly takes place in sensations, but which here is no longer sensation, because it is no longer conducted to the brain, the organ of consciousness. During health also numerous reflected motions result from stimuli of the skin, which do not come as true sensations to the con-

* There may certainly be considerable doubt respecting the action of the œsophagus—a doubt which nothing but careful experiment can solve. But I think I have proof that the *cardia* closes and opens upon the principle of the reflex function, as well as the pharynx, and that it is under the influence of the *internal excito-motory*, or pneumogastric nerve.—M. H.

sciousness, but still may excite violent impressions on the spinal marrow; as, for instance, the permanent contraction of the sphincters from the stimulus of the excrement and of the urine. But Dr. Marshall Hall goes too far, when he supposes that in health every motion on true sensation is induced by the will, and that all excitations of sensitive parts in the reflected motions are without sensation. For the reflected motions of sneezing, coughing, and many others follow actual sensations*.

"The reflected motions, and the involuntary not reflected motions are not to be confounded with one another. If the rima glottidis of an animal be touched, says Dr. Marshall Hall, a contraction takes place; the same, when the heart is touched. By removal of the brain no alteration ensues; but if the medulla oblongata be removed, the contractions of the larynx on stimuli cease, while those of the heart continue. The action of stimuli on the heart is an immediate one dependent on its irritability; a stimulus applied to the larynx must, on the contrary, be propagated to the medulla oblongata, and the contraction results indirectly from it. In a snake after the removal of the head a motion of the larynx ensued; it was drawn downwards and closed, as soon as Dr. Marshall Hall touched a spot within the teeth of the lower jaw or the nasal apertures. After removal of the medulla oblongata this ceased. Lastly, he mentions as belonging to the reflex function, the winking of the eyelids when they are touched; the peculiar action on the respiration by tickling, or when cold water is thrown into the face; sneezing from stimuli of the nasal mucous membrane; cough; vomiting from stimuli of the larynx or pharynx; tenesmus from stimulation of the rectum; and strangury from that of the bladder. We see that the spasms in diseases may have very different sources. There are, for instance, spasmodic affections which have their seat in the motor nerves themselves, and others which have their cause in the brain and spinal marrow; but there are also reflected spasms, whose cause lies in stimulation of sensitive nerves, as those which often take place after intestinal stimulation, in dentition, odontalgia, and painful nervous affections from organic and inorganic lesions generally.

"The phenomena which we have now described, first from our own observations and then from those of Dr. Marshall Hall, have all this in common with one another, that the spinal

* These and other acts of the excito-motory system are *attended by* sensation, but are not the less independent of it; some are entirely without it. —M. H.

marrow is the connecting link between a sensorial and a motorial motion of the nervous principle, though still the course which the conduction in the reflected motions from the sensitive to the motor nerves in the spinal marrow takes may be more definitely pointed out. The most common kind of reflected motion is, that the muscles of a limb, in which violent sensations have been excited, may be moved, as in the burning of the skin twitchings take place in the burned limb; or as in the commencement of the narcotization of an animal, on the sensitive stimulus of the skin the muscles of the stimulated limb are most easily moved; or as the morsels of food produce the reflected motion of the apparatus for swallowing; or as the particle in the conjunctiva exciting merely sensation, produces the reflected closure of the eyelids; or as, lastly, the stimulus of the urine and excrement act indirectly on the motion of the sphincters. As soon therefore as the sensitive-motion has reached the spinal marrow, it does not pass over the whole spinal marrow, but most easily to those motor nerves which have their origin nearest to the stimulated sensitive nerves; or in other words, the easiest way for the current or vibration is from the posterior root of a nerve or some of its primitive filaments to its anterior root, or to the anterior roots of several adjacent nerves. We see, then, that the nervous principle in these currents or vibrations takes the shortest way, acting from sensitive fibres through the medulla spinalis on motor fibres; just as electricity takes the shortest way from one pole to the other. More correctly expressed, and translated into physiological language, this means, that in violent excitation of the motor property of the spinal marrow through a sensitive nerve, that part only of the spinal marrow is first excited, and then excites movements, which gives origin to the sensitive nerve; and that the excitation of other parts of the spinal marrow, and the motor nerves arising therefrom, decreases in proportion as they are more removed from the spot excited by the sensitive nerve. The same holds also of the cerebral nerves, whose reflected phenomena appear to remain still almost quite unknown to Dr. Marshall Hall*. The great nerves of the senses are especially prone to cause reflected motions of the motor cerebral nerves, and especially the optic and auditory; they produce in vivid light and on loud sound a reflected excitation of the facial nerve, and thereby closure or winking of the eyelids. The optic nerve again easily produces the reflected excitation of the oculo-motor nerve in motion of the iris, and on looking at bright light it in-

* I am still of opinion that the reflex function is confined to the medulla oblongata and spinalis, exclusively of the brain.—M. H.

duces a reflected affection of the facial and other nerves in sneezing. But the great sensitive nerve of the anterior part of the head and the face, the great portion of the trigeminus, may excite the oculo-motor and facial through the medium of the brain; thus contraction of the iris takes place when cold water is thrown into the nose, and from tickling in the nose sneezing takes place, as well as the action of the facial nerve in the excitement of the facial muscles which is connected with it. In short, we see, that of the motor cerebral nerves, the part of the oculo-motor nerve which goes to the ciliary ganglion and thence to the iris, and the facial nerve, may most easily be excited by reflexion, and that the impressions either of sight, or touch, or hearing may be the exciting causes: therefore between the origins of the optic, trigeminus, and auditory nerves, and the points of origin of the motor nerves in the brain, there must be a facility of conduction pre-established by primitive formation. Those sensitive and motor nerves, whose mutual action is effected through the brain and spinal marrow, present a kind of balance with those central parts, one altering the other, as the ascent of one scale induces the descent of the other, or as the falling of a fluid in one leg of a bent tube produces the ascent in the other, till they are permanently at an equilibrium. If a sensitive nerve is not usually in a state to produce a reflected motion, yet on any violent impression on sensation it becomes so, and the brain and spinal marrow then reflect the currents or vibrations received from the sensitive nerves, into those motor nerves, to which the conduction from the sensitive fibres through the fibres of the brain and spinal marrow is most easy.

"Another very common path of conduction from the sensitive to the motor nerves through the medium of the spinal marrow and medulla oblongata, is that seen in the excitation of the mucous membranes and the secondary affection of the respiratory muscles in vomiting, evacuation of fæces, parturition, the coughing, sneezing, &c. Next to the above-mentioned law, that nerves of allied origins, or of not very remote origins, are peculiarly prone to the phenomena of reflexion, the most frequently acting law of the "Nervenstatik" is the reflexion now mentioned. Therefore, *in the medulla oblongata and spinal marrow*, between the sensitive nerves of the mucous membranes (the trigeminus in the nose; the vagus in the trachea, lungs, pharynx, œsophagus, stomach; the sympathetic in the intestinal canal and uterus; branches of the sacral plexus and sympathetic in the urinary bladder and rectum;) and the motor respiratory nerves (facial, accessory, and spinal nerves), there must be pre-formed easy means for a conduction; while, on

the contrary, the spinal nerves going to the extremities are excluded from this harmony.

"But if a certain irritation of the spinal chord and brain be induced by narcotism or other causes, then every perception may produce a discharge of the spinal marrow to all the motor nerves, even to those which are affected with the greatest difficulty, viz. the motor nerves of the extremities." (pp. 688—701.)

Such is the account of this subject given by Prof. Müller. I may be allowed to repeat that I have perused this unprejudiced and independent testimony to the importance of my investigations with unmingled satisfaction.

Before I dismiss the subject, I must add that my views are somewhat different from those of Prof. Müller:

1. I view the reflex function as the *distinct* and *peculiar* or *proper* function of the medulla spinalis, equally independent of the brain, the sympathetic, and of the anastomoses and the mere origins of nerves;

2. I regard this function as residing in the medulla, as the *axis* of a *distinct* system of *excitor* and *motor*, and *excito-motory* nerves;

3. I consider this function and its system of nerves as presiding over the *orifices* and the *exits* or *sphincters* of the animal frame, and over *ingestion* and *egestion*;

4. The brain is the central organ of sensation and volition, the organ of *mental* relation with the external world; the spinal marrow, on the contrary, is the central organ of excito-motory phenomena, and of the *physical* appropriation of certain external objects;

5. Respiration even is a part of this peculiar function: it is *excited* on ordinary, and on extraordinary occasions, through appropriate excitor nerves, especially the pneumo-gastric, but also the fifth and spinal nerves;

6. *Volition* may *modify* the acts of the reflex function, and these acts may be attended by sensation; but this function is, otherwise, independent both of volition and sensation, of their organ the brain, and of the mind or soul;

7. The *passions*, in an especial manner, demonstrate themselves through the medium of the true spinal marrow; and thus *pain* may induce surprise or fear, and *appear* to occasion an excito-motory act;

8. The brain *sleeps*; but the spinal marrow *never sleeps*;

9. Finally, the excito-motory system of nerves are the peculiar seat of action of certain *diseases*, and of certain *causes* and *remedies* of disease.

These and other propositions I am about to illustrate in a

series of papers preparing for the Royal Society. That they involve a principle in physiology at once extensive and novel, will not, I think, be now denied.

I may add that in some of the *Invertebrata*, the necessity for the nerves being *intervertebral* not existing, the excito-motory system of nerves, with their axis, may be as *distinct* in their anatomy as they are in their functions. This question I am about to subject to the test of experiment.

14, Manchester Square,
November 25, 1836.

XLIII. *An Analysis of* Dr. Ritchie's *Paper, in reply to* Mr. Rainey's *last Communication concerning Magnetic Reaction, contained in the Philosophical Magazine for January.* *By* G. Rainey, *M.R.C.S.**

THAT Dr. Ritchie is in error will be clear from the following facts. The expression most frequently employed to explain the action of a magnet upon its keeper is, that the magnet induces on the contiguous ends of the armature opposite states of magnetism, and that in consequence of this dissimilarity these oppositely magnetized extremities attract one another, evidently inferring that attraction follows as a consequence of induction. If a piece of soft iron be applied to magnets of different magnetic intensities, the adhesion would most undoubtedly be the greatest between the soft iron and the strongest magnet; and if any other magnet still stronger were applied to the same keeper, this increase would be manifested by a still more forcible attraction of this keeper by the magnet; and so on, without any known limit: consequently, as attraction is the effect of induction, the induction may be inferred to have no known limit, and the position to be true.

Dr. Ritchie says, "he, Mr. Rainey, also takes for granted, that a magnet having double the power will induce in the same armature twice the effect." I am not conscious of ever having made such an assertion.

Dr. Ritchie, to refute my first position and the one incorrectly attributed to me, has invented the following experiment:

"Roll a covered wire about the half of an electro-magnet A B. Do the same with an equal wire from C to D. Connect the first helix with an *elementary* battery, and ascertain the lifting power of the magnet. Connect the other helix with an equal battery, and instead of the lifting power being doubled, according to the principle assumed by Mr. Rainey, its power

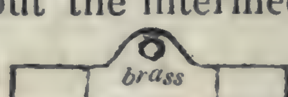
* Communicated by the Author.

may notbe increased a third or a fourth, or even a tenth, if the *battery* be a powerful one."—See Phil. Mag., January, p. 58.

From this experiment Dr. Ritchie not only endeavours to convince every person of the absurdity of my views, but so deeply is he impressed with the correctness of its indications that conclusions are deduced from it even with arithmetical precision.

Dr. Ritchie must know that if a bar of iron be under the influence of a galvanic current, the magnetism will accumulate in those parts of the bar nearest to the extremities of the wire, and that the part of the bar opposite the middle of the coil will be magnetically inactive; consequently when one leg was magnetized it would have two poles, and that pole adjacent to the inactive leg of the magnet would induce in it also polarity; consequently there would be two magnets instead of one. If the two coils were in action at the same time, each leg would be a separate magnet, and the magnetical condition of the intermediate curved part would depend upon the directions of the coils, and it might be a magnet also; consequently there would be three magnets together, the power of each being very uncertain. How far an experiment like this is calculated to furnish scientific truth, I must leave to the decision of your readers.

I now come to what Dr. Ritchie designates the second false principle. This is first called an assumption, and afterwards a deduction. This part of the subject relates to the reaction of the keeper upon the magnet. It was the subject of my first *short* paper, but has been discussed more fully in the subsequent ones.

The following experiments are directly connected with magnetic reaction, and are, I think, illustrative of the existence of such a property. Take two keepers of the same form. Let the one consist entirely of soft iron, and the ends of the other be made of iron, but the intermediate part of brass, as in the annexed diagram 

; apply these keepers separately to the extremities of a hard *single* horseshoe magnet well magnetized, and it will be found that the iron one holds, as nearly as can be estimated, double the weight of the iron and brass one. It will be perceived that the interrupted keeper will have its attractive force dependent entirely upon the induction of the poles of the magnet, and that the weight sustained will be a correct measure of the sum of the attraction of the poles, whilst the uninterrupted keeper will have its attraction doubled by the induction which takes place in consequence of the action of one end of the keeper upon the

other. On extending this experiment to compound magnets, I was at first surprised to find that the attraction of the ordinary keeper exceeded that of the interrupted one by considerably more than double. These keepers were applied to two compound magnets, the one having three plates, and the other five, of about the same size. The interrupted keeper was attracted nearly the same by each, but the common one was held much more firmly by the magnet having the greater number of plates. This brought to my recollection a fact which had often appeared very surprising, and for which I could find no explanation. After taking to pieces a compound horseshoe magnet consisting of eight blades, and of considerable sustaining power, I found that the quantity of magnetism in each blade was very trifling, and apparently far too little to account for the attractive power of the entire magnet. These magnets were each remagnetized to saturation, but the same reduction in their power took place on replacing them with their similar poles in contact. They were made of very soft steel, and after having their extremities hardened they lost much less by the contact of their similar ends: this improvement was not so perceptible on the application of the keeper to the whole magnet as to the individual parts. The explanation of these facts appears to be as follows: When magnets are placed with their similar poles in contact, they lose a considerable quantity of their magnetic influence. The absolute amount of loss will depend upon the temper of the steel; the harder it is the less it will lose: and also upon the number of magnets; the more there are the less will each magnet retain. Now, if the interrupted keeper be applied to a compound magnet, it will be attracted only with a force equal to the quantity of residual magnetism, which being very small the attraction will be comparatively feeble, though probably a correct measure of the absolute strength of the magnet. When the common keeper is applied, it will be obvious, from the double source of induction by which it is influenced, that its state of magnetism will exceed that of the weakened magnet, and by reacting upon it increase its power. The magnet having thus acquired an addition of magnetism will now react more forcibly upon the keeper, and the keeper in its turn upon the magnet, and so on, until the resistance from the contact of the similar poles, and the nature of the steel, cease to allow of any further induction from this source. The softness of the steel and the number of magnets being circumstances which favour the reaction of the keeper upon the magnet, are facts corroborative of the probability of this explanation, and show that the force with which the common keeper is attracted by a magnet is a fallacious indication

of its real power. A practical inference may be drawn from these experiments, namely, that little advantage is gained by the association of magnets, unless their hardness be such as to prevent the dissipation of so large a portion of their magnetism in consequence of the contact of their similar poles. Dr. Ritchie says that I have taken for granted that the action of a galvanic current upon a steel magnet is the same, whether its poles are connected by an armature or not. Certainly so far as the identity refers to the nature of the effect produced, there can be no occasion to entertain any doubt. The usual results of a galvanic current, such as polarity, &c., are precisely the same in each case, varying only in degree. It is the explanation of the difference in the quantity of effect of the galvanic current upon the steel when the poles are connected, and when separated, which forms the subject of discussion in my paper, and which has been attributed to the reaction of the keeper; therefore, so far from having adopted this as an axiom, I have endeavoured to deduce it as a consequence.

The last experiment which Dr. Ritchie adduces, p. 59, which is to set the matter at rest, is a very singular one. " Take a bar of steel, and bend another of the same size and length into a square; magnetize the straight bar by drawing it lengthwise over one of the poles of a magnet; move the same pole the same number of times round the square; break the bar into four equal parts, and the square at the corners, and the bar C D will be stronger than *either* (*any* is meant) portion of the straight bar." It will be obvious upon a little reflection, that in the process of magnetizing a closed circuit, of any form whatever, one part of the steel composing the circuit will act as a keeper to the other, and consequently that the bar thus magnetized is under the same circumstances as a horseshoe magnet would be when magnetized with a piece of iron in contact with its poles. Hence we see that Dr. Ritchie's square was under circumstances much more favourable for securing its full complement of magnetism than the bar was; therefore it is not to be deemed a matter of wonder that it was found to be so much stronger. The most curious part of this experiment is the laborious task of cutting each into four exactly equal parts; why, if Dr. Ritchie had found that the bar which had been bent was stronger than the straight one, surely he might have inferred that the parts of which the bent bar was made up would be stronger than those corresponding to them in the straight bar, and that the most troublesome part of the experiment might have been dispensed with. It is also equally strange why Dr. Ritchie should have chosen an exact square as the form necessary for the closed circuit. Does not

Dr. Ritchie know that the figure into which the bar might be bent would have nothing at all to do with the disposition of the magnetism through the metal? Of whatever form the bar might be, the instant the continuity was destroyed the magnetism would accumulate at the most remote extremities of the bar, leaving the centre inactive or neutral if the bar had been properly magnetized, and of the same form and density the neutral point would be exactly in the middle; the figure of a square having no influence whatever in making it otherwise.

As I am obliged to abridge my remarks as much as possible, I shall conclude my paper with this experiment, leaving your readers to judge of the merits or demerits of this discussion.

XLIV. *On the Results of Experiments made on the Weight, Height, and Strength of above* 800 *Individuals.* *By* James D. Forbes, *Esq., F.R.SS. L. & E., Professor of Natural Philosophy in the University of Edinburgh.**

THE interesting and remarkable experiments published by M. Quetelet, of Brussels, on various points of physical development in man, under a variety of circumstances, as to climate, station, age and sex, induced me to take the opportunity which my professional position presented of obtaining the measure of physical development as to the weight, height and strength of natives of Scotland between the ages of 14 and 25, students in our University.

In the prosecution of this plan separate lists were kept of persons not born in Scotland, and of these, the English and Irish lists have likewise been subjected to calculation. Though of these the numbers are comparatively small, the results present some pretty decisive characters. These experiments were continued during two winters (1834-5, 1835-6): every experiment was made by myself and noted down by myself. The weights were ascertained by Marriot's spring balance, which was verified from time to time and found to have undergone no change in its elasticity. The weight of clothes is included†. The heights are in English inches, shoes included. For the measure of strength, Regnier's dynamometer was employed, and these experiments were somewhat less satisfactory than the others. The error of the instrument had

* Read to the Royal Society of Edinburgh: and communicated by the Author.

† According to Quetelet, this amounts to $\frac{1}{18}$th of the weight.

been ascertained before the commencement of the experiments, and was found to be pretty constant throughout the scale. But after the experiments were finished this was by no means the case, the error having become variable owing to the interfering action of a small spring employed to bring the index to zero. As this, however, only affects the absolute results (or, at least, its relative influence is trifling), I have contented myself with applying an interpolated correction deduced from the mean of the errors before and after, which cannot differ much from the truth. But the instrumental errors are not the only ones to be contended with. To avoid errors in the *use* of the dynamometer requires vigilant superintendence on the part of the observer; and as the first pull is generally (though not always) greater than the second or third, this also must be allowed for. I have invariably repeated the experiment three times, and often much more frequently. When extraordinary cases have occurred I have taken the precaution of observing at distinct intervals of time.

In ascertaining the mean results the following method has been adopted: the natives of each country were separated, and each class divided, according to age, into twelve sets, from 14 to 25, the greatest number being of the age of 18 years. The mean weight, height and strength for each year was computed, and the result projected upon ruled paper. Curves were drawn through the points thus projected, in such a way as to represent most satisfactorily the whole observations. These curves, with the determining points, are now exhibited to the Society. It is proper to add that the ages registered being the ages *at last birthday*, the weight, &c., registered is not that due to the age noted, but at a mean to an age half a year later. Thus all the persons who were 20 last birthday are *between* the ages of 20 and 21, or 20½ at a mean. This has been attended to in making the projections.

Besides the English, Scotch and Irish curves, I have exhibited those of the Belgian development, from M. Quetelet's experiments, reduced to English measures. The thickness of the shoes not being included in these experiments, half an inch (perhaps too little) has been added to make them comparable with the others. It is important to add that M. Quetelet's experiments here quoted, as well as my own, were made upon persons in the higher ranks of life, in both cases, in fact, upon persons having the benefit of academical instruction.

The number of persons examined by me in the two winters before stated was thus divided: Scotchmen, 523; Englishmen, 178; Irishmen, 72; from the Colonies, &c. 56; total, 829.

I was careful to obtain a fair average of persons of all degrees of height and strength, in which respect the Scotch average is more unexceptionable than the others. There is always a tendency in such cases to get too high a development, because diminutive persons are the least likely voluntarily to come forward. An example of this is found in the mean height obtained by M. Quetelet, from a register of 80 individuals at Cambridge between the ages of 18 and 23, giving a mean of 69·6 inches instead 68·7 as my experiments indicate.

The numerical results derived from the graphical process before described are given at the close of the paper, and seem to warrant the following conclusions:

1. That in respect of weight, height, and strength there is a general coincidence in the form of the curves with those of M. Quetelet.

2. The British curves seem to have more curvature for the earlier years (14 to 17), or the progress to maturity is then more rapid, and somewhat slower afterwards. If we may depend upon the English curves, this is more strikingly the case in natives of that country than of Scotland, at least in point of weight and strength.

3. The tables incontestibly prove the superior development of natives of this country over the Belgians. The difference is greatest in strength (one fifth of the whole) and least in weight.

4. In comparing natives of England, Scotland, and Ireland more doubt arises, owing to the difference in the number of experiments; those for Ireland are confessedly most imperfect. Yet I conceive that the coincident results in the three tables entitle us to conclude that the Irish are more developed than the Scotch at a given age, and the English less. Some qualification is, however, due, in consequence of the remark (2.); for in the earlier years (14—17) it would even appear that the English so far get the start of the Scotch, as not only relatively but also absolutely to surpass them (in strength and weight); but between 17 and 19 they lose this advantage. I am disposed to think that this appearance of a result is not accidental.

5. The maximum height seems scarcely to be attained even at the age of 25. This agrees with M. Quetelet's observations. Both strength and weight are rapidly increasing at that age.

6. In the given period of life (14—26) all the developments continue to increase; and all move slowly from the commencement to the end of that period. Hence the curves are convex upwards. [This is not the case below the age of 14, for weight and strength. Quetelet.]

Tables.

Weights in Pounds (including clothes).

Age.	English.	Scotch.	Irish.	Belgians.
15	114·5	112	....	102
16	127	125·5	129	117·5
17	133·5	133·5	136	127
18	138	139	141·5	134
19	141	143	145·5	139·5
20	144	146·5	148	143
21	146	148·5	151	145·5
22	147·5	150	153	147
23	149	151	154	148·5
24	150	152	155	149·5
25	151	152·5	155	150

Heights in inches. Full dimensions (with shoes).

Age.	English.	Scotch.	Irish.	Belgians.
15	64·4	64·7	....	61·8
16	66·5	66·8	....	64·2
17	67·5	67·9	....	66·1
18	68·1	68·5	68·7	67·2
19	68·5	68·9	69·4	67·7
20	68·7	69·1	69·8	67·9
21	68·8	69·2	70·0	68·0
22	68·9	69·2	70·1	68·1
23	68·9	69·3	70·2	68·2
24	68·9	69·3	70·2	68·2
25	68·9	69·3	70·2	68·3

Strength in Pounds.

Age.	English.	Scotch.	Irish.	Belgians.
15	....	280	....	204
16	336	314	....	236
17	352	340	369	260
18	364	360	389	280
19	378	378	404	296
20	385	392	416	310
21	392	402	423	322
22	397	410	427	330
23	401	417	430	335
24	402	421	431	337
25	403	423	432	339

XLV. *On an Artificial Substance resembling Shell. By* Leonard Horner, *Esq., F.R.SS. Lond. and Edinb. With an Account of an Examination of the same. By Sir* David Brewster, *LL.D., F.R.S., &c.**

WHILE I was, some time ago, officially inspecting the cotton-factory of Messrs. J. Finlay and Co., at Catrine, in the county of Ayr, on going over the bleaching-establishment attached to it, I was struck with an unusual appearance of a part of the machinery, which, at a distance, looked as if it were made of brass. On a closer examination, I found that it was a large circular wooden box coated with an incrustation of a brown compact substance, having a highly polished surface, a metallic lustre, in some places beautifully iridescent, and when broken exhibiting a foliated texture†. This resemblance in structure and pearly lustre to some species of shells, such as the *Meleagrina*, *Malleus*, *Avicula*, *Ostrea*, *Pinna*, and others, induced me to examine the substance more closely, conceiving that it might possibly throw some light on the formation of shell.

The part of the machinery on which I observed the incrustation is called a Dashwheel, and consists of a circular box, about seven feet in diameter and three feet in width, revolving upon a horizontal axis, and having its interior divided into four compartments, into each of which there is a circular opening on one side. The purpose of this wheel is to wash or rinse the cloth in pure water, after it has been boiled or steeped in the bleaching-liquors. It makes twenty-two revolutions in a minute, which is found to be the proper degree of speed, in order that the cloth may be tossed about and *dashed* against the sides as the wheel turns; a greater velocity causing it to keep at the circumference without shifting its position.

I was told that the incrustation was a deposit of carbonate of lime, and the source of the lime was mentioned. But whence the brown colour, and the metallic nacreous lustre? If the substance were analogous to shell, it ought to contain animal matter; and whence could that be derived? It was necessary to trace the operations from the beginning.

The cotton cloth is brought to the bleach-field in the state in which it is taken from the weaver's loom. The first process is to steep it in water for several hours, after which it is immersed in cream of lime. This is made in the following manner: fresh-burned lime is slaked and passed through a fine

* From the Philosophical Transactions for 1836, p. 49.

† Specimens are deposited at the British Museum.

sieve, and added to water in the proportion of 38 lbs. of dry lime to 1000 lbs. of cloth. The cloth is boiled in this liquor from four to six hours, the lime acting as an alkali; and it is used only from being considerably cheaper than potash or soda. After this boiling, the cloth is taken to the dash-wheel to be thoroughly cleared of the lime, which is effected by its being tossed about for ten minutes in clear water in the interior of one of the compartments into which the wheel is divided. Here, then, is the source of the calcareous matter of the incrustation; and we have the lime dissolved or suspended in the water in a state of extremely minute division, and from which it is deposited, most probably, by a partial evaporation. It is difficult to say whether the deposit takes place while the wheel is revolving, by the water being broken into a kind of spray, and so presenting a greater surface for evaporation, or during the night, when the wheel is still: some of the properties, to be afterwards described, render the latter supposition the most probable. But in whatever way it takes place, the operation is an exceedingly gradual one; for the wheel had been in constant use for ten years, and the coating in the interior did not exceed one tenth of an inch in thickness. It had been in operation about two years before any perceptible deposit showed itself in the inside; but it had not been going half a year before an incrustation began to be formed on the outside of the wheel. I remarked that the deposit was in greatest quantity around the orifice where the cloth is put in and taken out. The deposit in the interior, and which coated the whole surface of the compartment, was of a darker brown colour, and was as smooth and splendent as a lining of highly polished bronze would have been. The high polish is no doubt partly produced by friction; and I observed that it was highest on that part of the outside nearest the opening.

So far we have *calcareous*, but no *animal*, matter; but in going a little further back in the history of the process to which the cotton had been subjected, before it came to the bleachfield, I discovered that animal matter might be contained in the incrustation. I learned that the cloth had been woven in power-looms; and on making inquiry as to the composition of the dressing or paste used to smooth and stiffen the warp before it is put into the loom, I was told that in the factory from whence the cloth had come, it is the practice to mix glue with the wheaten flour, generally in equal proportions by weight.

We have thus lime and gelatine, the same materials which are employed by the molluscous animal in the formation of its covering, and apparently in the same degree of minute division as that in which they are exuded from its mantle.

Chemical examination of the Substance.

1. *The external deposit.*—Exposed to the flame of a wax candle, it blackens, and gives out the usual smell of burning animal matter, the thin laminæ of which it is composed separating and curling up like films of horn; appearances similar to those exhibited by membranous shells when heated. When the flame is urged by the blowpipe, the laminæ separate still more, and are changed into an extremely light and brittle enamel, pure white, and having a pearly lustre. A fragment moistened on the back of the hand gives a sensation of heat, as quicklime does when so treated. The substance, when thrown into dilute muriatic acid, is entirely dissolved; the fluid is tinged yellow, and the effervescence produces a froth, like beer. When the acid is very much diluted, and a portion of the substance is suspended in it, the solution takes place gradually, minute flocculi of animal matter being separated, and floating in the fluid.

2. *The internal deposit.*—This is separable into extremely thin laminæ, and these, when in small fragments, are hardly distinguishable from scales of brown mica, showing also the most beautiful play of colours. The action of heat produces the same effect as on the external deposit, except that the separated laminæ are thinner. The action of muriatic acid is the same, but the yellow tinge is deeper, and the froth is more permanent, indicating a larger proportion of animal matter than in the other. The nacreous lustre is also much more conspicuous in this.

Mr. Gray, in his paper on the Structure of the Shells of Molluscous Animals, observes that the pearly or iridescent lustre appears to be confined to shells of the concretionary structure, which when broken exhibit a nearly uniform texture, but separate when heated into numerous thicker or thinner laminæ; and he adverts to the observation of Mr. Hatchett, that when they are digested in weak muriatic acid, the lime is dissolved, leaving a great number of thin plates of animal matter, which retain the original shape of the shell. He adds, "This variety of structure is found to constitute the whole shell of the *Anomiæ* and *Placunæ*, and to form the inner coat of those shells which have pearly insides, as the *Turbines*, *Haliotides*, *Uniones*, &c., as well as the laminar portion of the *Pinnæ* and Mother-of-pearl shells *."

Besides the laminated structure, there is, in the case of the

* Phil. Trans., 1833, p. 794. [An abstract of Mr. Gray's paper here referred to will be found in Lond. and Edinb. Phil. Mag., vol. iii. p. 452. Mr. Hatchett's paper was reprinted entire in Phil. Mag., First Series, vol. vi. p. 21.—Edit.]

Pinna and some other shells, a prismatic crystalline arrangement of the particles perpendicular to, and passing uninterruptedly through, the laminæ; but I have not discovered such an arrangement in any portion of the incrustation, even when examined by the microscope.

I felt very desirous that this singular deposit should be examined by Sir David Brewster; the more especially as he had long since directed his attention to the peculiar structure of mother-of-pearl *. On showing him the specimens in my possession he observed, that it was one of the most remarkable artificial productions he had seen; and he readily undertook to examine it carefully. He shortly afterwards sent me the particulars of that examination, which had afforded some curious and interesting results. Having subsequently visited Catrine, I procured more perfect specimens; and I sent these to Sir David Brewster, in order to ascertain whether they might not afford something new, in addition to the results he had obtained from the fragments he had formerly examined. They did so, and I now subjoin the very interesting account which Sir David has given me of the properties he has discovered in this new substance.

My dear Sir, Belville, Jan. 1st, 1836.

In the communication which I had the pleasure of addressing to you on the 20th of January, 1835, I gave a brief account of the observations I had made on the highly interesting substance which you had put into my hands; but as the specimens which you sent me a few days ago are so much superior to those with which I made my former experiments, and have led me to some new and I think rather extraordinary results, I shall include in the present letter all my former observations.

The substance in question does not resemble in its general aspect any natural or artificial production which I have seen. It is, generally speaking, brown where the surface is not iridescent, and in very thin plates: it is almost perfectly transparent, with a slight yellowish brown tinge like plates of glue or lac of the same thickness. The laminæ of which it is composed are sometimes separated by vacant spaces, at other times slightly coherent, but generally adhering to each other with a force greater than that of the laminæ of sulphate of lime or mica, and less than those of calcareous spar. When the adhering plates are separated, the separated surfaces are sometimes colourless, especially when these surfaces are corrugated or uneven; but they are almost always covered with an iridescent film of the most brilliant, and, generally, uniform tint,

* Phil. Trans., 1814.

which exhibits all the variety of colours displayed by thin plates or polarizing laminæ.

The substance is of intermediate hardness between calcareous spar and sulphate of lime. It scratches the latter easily, and is not scratched by mother of-pearl. Its specific gravity is shown in the following Table, which indicates its relation to analogous substances.

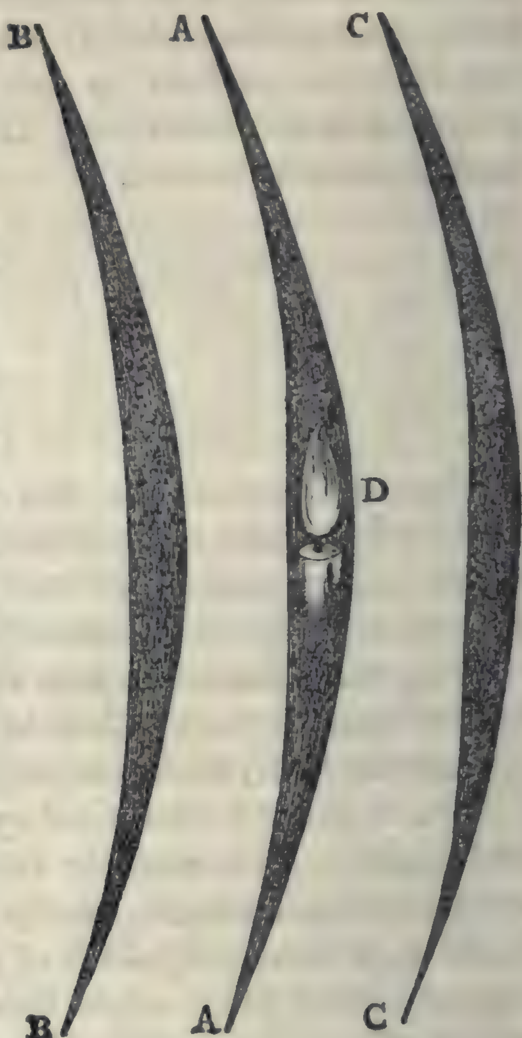
Calcareous spar	2·72
Oriental pearls	2·68
New substance	2·44
Mother-of-pearl	2·19
Oyster-shell	2·02.

The new substance has the property of refracting light doubly, like most crystallized bodies; and, as in agate, mother-of-pearl, &c., one of the two images is perfectly distinct, while the other contains a considerable portion of nebulous light, varying with the thickness of the plate and the inclination of the refracted ray. It has one axis of double refraction, like calcareous spar, which is negative, as in that mineral, and, like it also, it gives a beautiful system of coloured rings by polarized light. The double refraction of the substance is very considerable, though greatly less than that of calcareous spar. A plate, one seventy-fifth of an inch thick, makes the first red ring of the system *eight* inches in diameter at a distance of twenty-six inches from the eye. The substance belongs to the rhombohedral system, and, as in the *Chaux carbonatée basée* of Hauy, the axis of the rhombohedron, or that of double refraction, is perpendicular to the surface of the thin plates. As mother-of-pearl has two axes of double refraction like aragonite, this new substance may be considered as having the same optical relation to calcareous spar that mother-of-pearl has to aragonite.

When we look through a plate of this substance perpendicularly to its surface, or along the axis of double refraction, the flame of a candle is seen encircled with a nebulous haze like a halo. By the slightest inclination of the plate in any azimuth whatever, three elongated and curved nebulous images are seen, as in fig. 1., the central one, A A, having a distinct image, D, of the candle in the middle of it, and the other two, B B and C C, being equidistant from A A. These elongated images are parallel and concave towards the end of the plate nearest the eye. In the direction of the axis of double refraction, when all the nebulous light is in one mass, the distinct image, D, is redder than in any other direction; and by slightly inclining the plate the red light disappears, and the

distinct image becomes brighter and whiter. All the three images, A A, B B, and C C, are united into a mass round D, at a perpendicular incidence, but they separate upon inclining the plate, and their distance increases with the inclination.

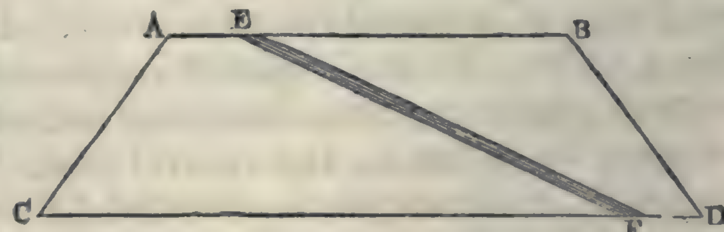
Fig. 1.



If we examine the nature of the light of which these images are composed, we shall find that the nebulous images, A A, B B, are wholly polarized in a plane passing through the direction of their length, while C C and the greater part of D are polarized in an opposite plane. As the thickness of the plate increases, more and more of the distinct image, D, is polarized in the same plane, as in mother-of-pearl *, till at a certain thickness the whole of it is thus polarized. In this case all the doubly refracted light which forms the nebulous image, A A, and the bright one, D, consists of two oppositely polarized pencils, the one forming the nebulous and the other the distinct image.

In investigating the cause of these phænomena, we must take as our guide the analogous facts presented by certain composite crystals of calcareous spar. Having long ago described this class of phænomena very fully †, I shall only state at present the general fact. Let A B C D, fig. 2., be a section of a rhomb of calcareous spar having its axis perpendicular to the faces A B, C D, and let E F be another crystal, or vein of the same substance crossing it according to the law of crystallographic composition. If we now look at a candle through this compound crystal, it will appear single in the direction of the axis of A B C D; but if we incline the plate in

Fig. 2.



* See Phil. Trans., 1814.

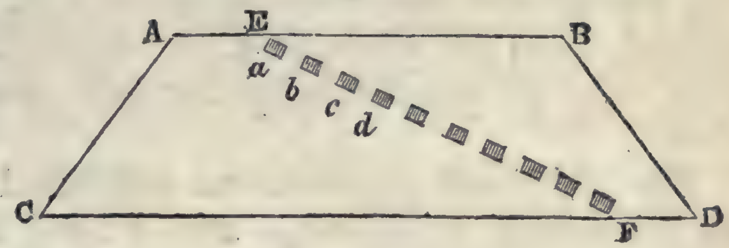
† *Ibid.*, 1815. Edinburgh Encyclopædia, art. Optics.

a plane passing through A B, we shall see two images together, as at A and D, fig. 3., and other two, namely, one at B and the other at C. These images, B, C, separate by the inclination of the plate exactly like those in fig. 1., and all the four, A, B, C, and D, have the same absolute and relative polarization as the four analogous images seen through the new substance, with this difference only, that none of them are nebulous.

Fig. 3.

If we conceive the vein E F to consist, as in fig. 4., of a great number of small crystals, *a*, *b*, *c*, *d*, &c. in place of one, the very same effects will be produced.

Fig. 4.



When we look through the new substance, the multiplication of images takes place *in whatever azimuth* we incline the plate, the elongated images being always perpendicular to the azimuth of inclination. Hence it follows, that these images are produced by *numbers of minute crystals* lying in or near the azimuth in which the plate is inclined; and that these crystals have their axes all inclined to that of the plate which contains them, at the same angle as the vein E F, figg. 2 and 4, is inclined to the axis of the rhombohedron of Iceland spar. But the remarkable result of these observations is, that in place of one set of crystals, or sometimes three sets, which occur in calcareous spar in three different azimuths, we have here *an infinite number of them* lying in every possible azimuth, and these so small in their dimensions that they cannot be recognised by the most powerful microscopes, except as dark specks disseminated through the general mass; and yet they indicate by their action on light, not only their existence, but the position of their axes, and their doubly refracting and polarizing structure, as unequivocally as if we could handle them, and cleave them, and place them upon the goniometer.

It may now be asked why the images are nebulous, and not distinct as in calcareous spar. The reason is, that the substance is imperfectly crystallized like the agate, mother-of-pearl and other bodies in which the doubly refracting force separates the incident light into two oppositely polarized pencils, which are not perfectly equal and similar, but which differ from each other, sometimes in the intensity of their light, sometimes in the distinctness of the image, sometimes in the nature or brightness of the colour, and sometimes in more

than one of these characters. But though the new substance resembles the crystals above mentioned in giving dissimilar pencils of doubly refracted light, it stands unique among all bodies with which I am acquainted in possessing the extraordinary system of composite crystallization, in which an infinite number of crystals are disseminated equally in every possible azimuth through a larger crystalline plate, having their axes all inclined at the same angle to that of the larger plate, and producing similar phænomena in every direction, and through every portion of the plate; or we may describe this remarkable structure by saying that the minute elementary crystals form the surfaces of an infinite number of cones whose axes pass perpendicularly through every point of the larger plate*.

The iridescent phænomena exhibited by the new substance are extremely interesting, and I have been at much pains to examine them in a great number of specimens. The plates into which the substance is divisible have been formed in succession, and certain intervals of time have elapsed between their formation. In general every two contiguous laminæ are separated by a thin iridescent film, varying from the three to the fifty millionth part of an inch in thickness, and producing all the various colours of thin plates which correspond to intermediate thicknesses. Between some of the laminæ no such film exists, probably in consequence of the interval of time between their formation being too short; and between others the film has been formed of unequal thickness, as happens in the oxidations upon steel when they are formed upon or around hard parts of the metal called *pins* by the workmen.

There can be no doubt that these iridescent films are formed when the dash-wheel is at rest during the night, and that when no film exists between two laminæ, an interval too short for its formation (arising perhaps from the stopping of the work during the day,) has elapsed during the drying or induration of the one lamina and the deposition of the other.

That these iridescent films are not thin films of the substance itself, may be inferred from the fact that light is reflected from their surfaces when they firmly adhere to the laminæ which inclose them. If, for example, we remove or raise up from a piece of mica a thin film which gives a bright green tint, and press it again into optical contact with the surface from which it was separated, it will then cease to exhibit any

* A rude idea of this structure is given by the beautiful cones, or rather pyramids of microscopic crystals of titanium which I have somewhere described as existing within the pyramids of many crystals of amethyst from the Brazils.

colour, because no light is reflected from its posterior surface; but if we press it into optical contact with another surface which has a different refractive power, its green colour will still be exhibited. It is owing to this cause that the colours of the oxidations on steel are so distinctly visible, and that the analogous oxidations are seen upon glass even before the film has begun to separate into coloured scales.

The iridescent films in the new substance possess another source of interest, in so far as they promise to throw a new light on the origin of the *incommunicable* colours of mother-of-pearl, which arise from the interior structure of the shell, and which cannot therefore be communicated to wax. These colours have frequently occupied my attention since the year 1814, when I described the phænomena of the colours communicable to wax*; but though I have devoted much time to the inquiry, I never could obtain a single result worthy of being communicated to the public. I took plates of mother-of-pearl that exhibited different bright colours through different parts of their surface, and by getting the mother-of-pearl ground away in different places by the seal-engraver's wheel, I endeavoured to discover the thicknesses at which the colours were produced, and the cause of the capricious variation of tints which arose from every inclination of the plate: but all my experiments were fruitless, and I abandoned the subject as beyond my reach. The phænomena, however, presented by the new substance seem to me to disclose the secret of which I was in quest. The layers of mother-of-pearl are deposited in succession like those which are formed upon the dash-wheel; and there can be no doubt that the animal whose mucous secretions form the shell that incloses it, rests occasionally from its toils, and affords a sufficient interval for the formation of an iridescent film upon the surface of the plate of shell which it daily deposits. Owing to the firm adhesion of the successive layers of the shell, we cannot, as in the more imperfectly formed new substance, separate each stratum in order to see the iridescent film upon their surfaces; but we can easily determine what phænomena would be produced if the layers of the new substance were as transparent as those of mother-of-pearl. If this were the case, we should see, both by reflected and transmitted light, the combined colours of all the iridescent films in the plate. When these films are numerous and flat, and of various thicknesses, the union of all their colours would form a pearly whiteness by reflected light, and when films of a particular colour predomi-

* Philosophical Transactions, 1814.

nate, both the reflected and the transmitted light would exhibit that prevailing colour: but if their surfaces are undulated as in mother-of-pearl, from the form of the shell and other causes,—if the iridescent films vary in thickness, and consequently in colour,—if they are wanting in some parts of the shell, and abound in others,—and if films of equal thicknesses occur in several laminæ in succession, and films of other thicknesses in other laminæ, which must necessarily take place from the varying and remitting action of the animal agent, then we shall have the very structure which is necessary for the production of the incommunicable colours of mother-of-pearl.

I have no doubt that this is the true cause of the phænomena which had so long perplexed me; and the results which I formerly obtained, though I could then attach no meaning to them, are in perfect unison with the preceding views. In order, however, to obtain something like an experimental confirmation of this opinion, I have examined the *fracture* of a mother-of-pearl shell where the laminæ have been all deposited with considerable regularity, and where their overlying edges are exhibited, and I find distinct and positive proofs of the existence of iridescent films, sometimes green, and sometimes red in several successive strata.

I am, my dear Sir,
Ever most truly yours,
D. Brewster.

To Leonard Horner, Esq.

XLVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Nov. 17, 1836.—"RESEARCHES in the Integral Calculus." Part II By Henry Fox Talbot, Esq., F.R.S.

Having explained, in the first part of his paper, a general method of finding the sums of integrals, the author proposes, in the second place, to apply this method to discover the properties of different transcendents, beginning with those of the simplest nature. With this view, he first shows its application to the arcs of the circle and the conic sections; and demonstrates the possibility of finding three arcs, such that, neglecting their signs, the sum of two of them shall be equal to the third, though not superposable in any part: an equality which has been hitherto deemed impossible in the ellipse and hyperbola, without the addition of some algebraic quantity.

November 24, 1836.—"Investigation of New Series for the Rectification of the Circle." By James Thomson, LL.D., Professor of Mathematics in the University of Glasgow. Communicated by Francis Baily, Esq., V.P. and Treasurer R.S.

The author obtains formulæ by which the ratio of the circumference of a circle to its diameter may be computed with much greater facility and expedition than by any of the ordinary methods.

A paper was also in part read, entitled, "Inquiries respecting the Constitution of Salts. Of Oxalates, Nitrates, Phosphates, Sulphates, and Chlorides." By Thomas Graham, Esq., F.R.S. Ed., Professor of Chemistry in the Andersonian University of Glasgow, &c. &c. Communicated by Richard Phillips, Esq., F.R.S.

A Report upon a Letter addressed by M. le Baron de Humboldt to His Royal Highness the President of the Royal Society, and communicated by His Royal Highness to the Council, was also read.

Nov. 30, 1836. *Anniversary Proceedings.* Having inserted the Address of H. R. H. the President in our last number, p. 141, we now complete our report of these proceedings.

Extracts from the Report of the Proceedings of the Council since the last Anniversary.

The Council, on the 3rd of March, adopted a Report, submitted to them by the Committee whom they had appointed for considering the communications from the Treasury and Excise Office, on the subject of the construction of instruments and tables for ascertaining the strength of spirits, in reference to the charge of duty thereon, and ordered it to be transmitted to the Lords Commissioners of His Majesty's Treasury; who in acknowledging its receipt, were pleased to express "their best thanks to H. R. H. the President, and to the Society, for the obliging manner in which they had met the wishes of the Board, and to the Committee for the attention they gave to the subject, and for the valuable Report with which they had furnished that Board."

The Council, conformably with the recommendation of the Donation Fund Committee, have granted £50 from that fund to Professor Wheatstone, in aid of the experimental inquiry which he is prosecuting on the measure of the velocity of Electricity when passing along a conducting wire.

A letter from Baron Von Humboldt, addressed to H. R. H. the President, relating to a proposal for the cooperation of the Royal Society in carrying on an extensive series of magnetical observations, in various parts of the earth, having been communicated by H. R. H. to the Council, it was referred to the Astronomer Royal and to S. H. Christie, Esq., for their opinion thereupon. The Report of these Gentlemen was ordered to be read to the Society and printed in its proceedings; and a Committee has been appointed to consider of the best means of carrying into effect the measures recommended in that Report*.

Mr. Monk Mason having, in a letter addressed to H. R. H. the President, offered the Great Vauxhall Balloon for the use of the Society, a Committee was appointed to take this proposition into consideration and to report thereupon to the Council.

* A Translation of M. de Humboldt's letter will be found in our last volume, p. 42.—Edit.

The Council have awarded a Copley Medal to Baron Berzelius for his application of the Doctrine of Definite Proportions in Determining the Constitution of Minerals. To the labours of this distinguished chemist, science is indebted for many of the facts by which the Laws of Definite Union were established. As early as 1807, soon after Dalton and Gay-Lussac had made known their views on this vital branch of modern chemistry, Berzelius commenced an elaborate examination on the proportions in which the elements of compound bodies are united, beginning with the salts, and subsequently extending his researches to all other departments of his science, as well to the products of organized existences as to those of the mineral world. The first part of the inquiry appeared in a series of essays in the Afhandlingar i Fysik, Kemi, och Mineralogie, t. iii. iv. v. and vi., as also in the Memoirs of the Academy of Sciences of Stockholm, for the year 1813. Since that period he has almost constantly been more or less occupied with researches bearing on, or illustrative of, the same subject. His numerous analyses of minerals enabled him at once to elucidate their nature through the light derived from the laws of definite combination, and at the same time to supply in the composition of minerals a splendid confirmation of the universality of those laws. It is for this branch of his inquiry that the Copley has been awarded.

A Copley Medal is also awarded to Francis Kiernan, Esq., for his discoveries relative to the Structure of the Liver, as detailed in his paper communicated to the Royal Society, and published in the Philosophical Transactions for 1833.

Before the researches of Mr. Kiernan, the liver was supposed to consist of two dissimilar substances, composed of brown parenchymatous granules, contained in a yellow substratum. The relation of the vessels and excretory ducts to these supposed dissimilar substances was not known; nor, although the organ was considered to be a conglomerate gland, were the glandules of which it was conjectured to be composed, defined in magnitude, shape, or disposition. Mr. Kiernan's discoveries show that in place of two textures there exists but one; and that the difference of colour results from the accidental congestion of one or other of the systems of vessels, which are found in the liver. Mr. Kiernan has further satisfactorily demonstrated the size and limits of the integral glandules of which the liver consists. He has traced the relation to these glandules of the different orders of vessels, which are distributed through the organ, and has explained the mechanism of biliary secretion. He has shown that all the blood employed in secreting bile is venous; and that the origins of the biliary ducts differ in an important respect from the origins of the ducts of all other glands: inasmuch as they form a series, not of coiled or branching tubes, but of anastomosing vessels, constituting a tubular network.

Mr. Kiernan's researches display great industry and ingenuity; when foiled by the difficulties which had foiled preceding anatomists, he applied a principle that had not been thought of before to facilitate the investigation of structure. Hitherto, however eminent the En-

glish have been in physiology, (and the most eminent of physiologists, Harvey, was an Englishman,) they have been behind the Germans and the Italians in anatomy. The discovery which Mr. Kiernan has made, exceeds in originality, and in importance is scarcely inferior to, any single anatomical discovery on record. Its originality consists in this; it may be estimated from the circumstance that nothing which had been previously done on this subject affords a clue to what *he* has found; and the difficulty of the inquiry may be understood from this; that although many had undertaken it, all had previously failed. The importance of the facts displayed may be gathered from the consideration, that they greatly elucidate the morbid anatomy of the liver,—a part of the human frame, which is remarkable for the frequency and variety of its diseases, and at the same time for the facility with which it may be influenced by remedial agents.

The Royal Medal for the present year, which the Council had proposed to give to the most important paper in Astronomy communicated to the Royal Society within the last three years, is awarded to Sir John Frederick William Herschel, for his Catalogue of Nebulæ and Clusters of Stars, published in the Philosophical Transactions for 1833.

In delivering this Medal His Royal Highness addressed the Society as follows:—

This, Gentlemen, is the second time that a Royal Medal has been adjudged to Sir John Herschel, for researches in a department of Astronomy which has descended to him as an hereditary possession; and I believe I may venture to say, that in no case has a noble inheritance been more carefully cultivated or more enriched by new acquisitions. The catalogue for which the Royal Medal is now given, contains a list of 2500 nebulæ and clusters of stars, the same number which had been observed and catalogued by his father, though only 2000 of them are common to both catalogues; the right ascensions and declinations of all these objects are determined; the general character of their appearance recorded; and all those which present any very extraordinary character, shape, or constitution, of which there are nearly 100, are drawn with a delicacy and precision which is worthy of an accomplished artist. It presents a record of those objects so interesting as forming the basis of our speculations on the physical constitution of the heavens which are observable in this hemisphere, which is sufficiently perfect to become a standard of reference for all future observers, and which will furnish the means of ascertaining the changes, whether periodical or not, which many of them are probably destined to undergo. I trust, Gentlemen, that a long time will not elapse before we shall be enabled to welcome the return of Sir John Herschel to this country, with materials for a catalogue of the nebulæ of the southern hemisphere as perfect and as comprehensive as that which we are this day called upon to signalize with the highest mark of approbation which it is in our power to bestow. He will then have fixed the monuments of an imperishable fame in every region of the heavens.

The Royal Medal for the present year, which the Council had proposed to give to the most important paper in Animal Physiology communicated to the Royal Society within the last three years, is awarded to George Newport, Esq., for his series of investigations on the Anatomy and Physiology of Insects, contained in his two papers published in the Philosophical Transactions within that period.

Mr. Newport, to whom the Society was indebted in 1832 for a very valuable and elaborate anatomical investigation of the nervous system of the *Sphinx ligustri* of Linnæus, and of the successive changes which that insect undergoes during the state of larva, and the earlier stages of the pupa state, published in the Philosophical Transactions of that year, has since prosecuted this arduous and laborious train of inquiry, under circumstances of peculiar difficulty, with extraordinary zeal and indefatigable perseverance. Within the period of the last three years he has enriched the Transactions with two papers, in the first of which, read to the Society in June 1834, he has extended his researches into the structure and arrangement of the different portions of the nervous system of the same insect, following their successive changes through the remaining stages of development to the completion of the imago, or perfect state. He devotes particular attention to the study of the periods at which those several changes occur; for he has found that they vary considerably in the rapidity of their progress at different epochs, according as the vital powers are called into action by external influences, or as they become exhausted by their efforts in effecting the growth, or modifying the form of different systems of organs. The labours of Mr. Newport have determined, with great exactness, those periods, which had not before been ascertained.

Among the numerous original observations of Mr. Newport on the arrangement and connexions of the several parts of the nervous system, the description he gives of the origin and distribution of the visceral nerve, which he shows to be analogous to the pneumogastric nerve of vertebrated animals, and also of the system of nerves corresponding to those which have been considered as peculiarly subservient to the supply of the respiratory organs, are particularly deserving of notice. In the course of this investigation many new and important facts are brought to light, which had escaped the observation of Lyonet, Müller, Brandt and Straus-Durkheim. Mr. Newport has also traced a remarkable analogy in the origin and distribution of the two distinct classes of nerves, the one subservient to sensation, and the other to volition, belonging to insects, with those belonging to vertebrated animals, and has thus given greater extension to our views of the uniformity existing in the plans of animal organization than we before possessed, and which are thus made to comprehend the more minute, as well as the larger tribes of the animal creation.

In a memoir on the Respiration of Insects, more recently communicated to the Society, and of which, at its last meeting in June, the title only could be announced, Mr. Newport has, with great diligence

and success, investigated the variations occurring in this function in the different periods of insect development. He has minutely traced the several changes which the tracheæ and spiracles undergo during the transformations of the insect, and has particularly described the successive development of the air-vesicles in connexion with the power of flight. He has given a minute and accurate description of the system of muscles, both of inspiration and of expiration, of the *Sphinx ligustri*; has investigated their various modes of action, with reference more especially to the different classes of nerves appropriated to these functions; and has established a distinction in the offices of these nerves, corresponding to the sources from which they derive their origin, and presenting remarkable analogies with similar distinctions in the nerves of vertebrated animals. He has given the result of a series of original experiments on the products of respiration in this class of animals, and of their variations under different circumstances of temperature, of submersion, and of confinement in unrespirable and deleterious gases; and he has deduced important conclusions relative to the circumstances which govern the properties of oxygen consumed and of carbonic acid generated. He has also communicated various results to which he has arrived concerning the capabilities which insects possess of supporting life during longer or shorter periods, when immersed in different media.

For the original views presented in these two papers, as well as for the mass of valuable information they contain, the results of much laborious and well-directed research in the more difficult departments of the Anatomy and Physiology of Insects, prosecuted under circumstances which would have repressed the exertions of a less ardent inquirer into truth, the Council have considered Mr. Newport as highly deserving the distinction they have conferred upon him by the award of the Royal Medal for Animal Physiology in the present year.

The Council propose to give one of the Royal Medals in the year 1839 to the most important unpublished paper in Astronomy communicated to the Royal Society for insertion in their Transactions after the present date, and prior to the termination of the Sessions in June 1839.

The Council propose to give one of the Royal Medals in the year 1839, to the most important unpublished paper in Physiology, communicated for insertion in their Transactions after the present date, and prior to the termination of the Sessions in June 1839.

The ballot for Officers and Council being taken, the following was the result:

President: His Royal Highness the Duke of Sussex, K.G.—*Treasurer:* Francis Baily, Esq.—*Secretaries:* Peter Mark Roget, M.D.; John George Children, Esq.—*Foreign Secretary:* Charles Konig, Esq.

Other Members of the Council: George Biddell Airy, Esq., A.R.; William Allen, Esq.; John Bostock, M.D.; The Earl of Burlington; Samuel Hunter Christie, Esq.; Viscount Cole, M.P.; Joseph Henry Green, Esq.; George Bellas Greenough, Esq.; William Lawrence,

Esq.; John Lindley, Phil. D.; John William Lubbock, Esq., M.A.; Rev. George Peacock, M.A.; William Hasledine Pepys, Esq.; Rev. Adam Sedgwick, M.A.; William Henry Smyth, Capt. R.N.; William Henry Fox Talbot, Esq.

December 8, 1836.—A paper was read, entitled, "Inquiries respecting the Constitution of Salts. Of Oxalates, Nitrates, Phosphates, Sulphates, and Chlorides." By Thomas Graham, Esq., F.R.S. Edin., Professor of Chemistry in the Andersonian University of Glasgow, Corresponding Member of the Royal Academy of Sciences of Berlin, &c. Communicated by Richard Phillips, Esq., F.R.S.

The results which the author had obtained from his former experiments, and of which he communicated an account to the Royal Society*, suggested to him the probability that the law with respect to water being a constituent of sulphates, would extend also to any hydrated acid and the magnesian salt of that acid. As he had already found that the sulphate of water is constituted like the sulphate of magnesia, so he now finds the oxalate of water to resemble the oxalate of magnesia, and the nitrate of water to resemble the nitrate of magnesia. His researches render it probable that the correspondence between water and the magnesian class of oxides extends beyond their character as bases; and that in certain subsalts of the magnesian class of oxides, the metallic oxide replaces the water of crystallization of the neutral salt, and discharges a function which was thought peculiar to water. In the formation of a double sulphate, the author finds that a certain degree of substitution or displacement occurs; such as the displacement of an atom of water pertaining to the sulphate of magnesia, by an atom of sulphate of potash, to form the double sulphate of magnesia and potash. The same kind of displacement appears to occur, likewise, in the construction of double oxalates; and the application of this principle enables us to understand the constitution both of the double and super-oxalates, and to explain the mode of their derivation.

The author then proceeds to apply these principles to the analysis of the oxalates; and 1st, of the oxalate of water, or hydrated oxalic acid; 2ndly, of oxalate of zinc; 3rdly, of oxalate of magnesia; 4thly, of oxalate of lime; 5thly, of oxalate of barytes; 6thly, of oxalate of potash; 7thly, of binoxalate of potash; 8thly, of quadroxalate of potash; 9thly, of oxalate of ammonia; 10thly, of oxalate of soda; 11thly, of binoxalate of soda; and lastly, of the double oxalates, such as, 1st, oxalate of potash and copper; 2ndly, oxalate of chromium and potash; 3rdly, oxalate of peroxide of iron and potash; and 4thly, of oxalate of peroxide of iron and soda.

In the second section he treats of the nitrates; and 1st, of hydrated nitric acid, or the nitrate of water; 2ndly, of nitrate of copper; 3rdly, of subnitrate of copper; 4thly, of nitrate and subnitrate of bismuth; 5thly, of nitrate of zinc; 6thly, of nitrate of magnesia; and 7thly, of supposed double nitrates and supernitrates. He con-

* See Lond. and Edinb. Phil Mag., vol. iii. p. 451.

cludes, from his experiments on this subject, that there is no proof of the existence of a single supernitrate.

In the third section he discusses the constitution of the phosphates. Phosphoric acid, he observes, is quite peculiar in being capable of combining with bases in three different proportions; forming, besides the usual class of monobasic salts, containing one atom of acid to one atom of protoxide as base, two other anormal classes of salts, in which two or three atoms of base are united to one atom of acid, namely, the pyrophosphates and the common phosphates, as they are usually denominated, but which the author proposes to designate by the terms, *bibasic*, and *tribasic* phosphates. Arsenic acid forms only one class of salts; but that class is anormal; every member of it containing three atoms of base to one atom of acid, like the common, or tribasic, phosphates. These anormal classes of phosphates and arseniates, with, perhaps, the phosphites, are, the author believes, the only known salts to which the ordinary idea of a subsalt is truly applicable: all other reputed subsalts being probably neutral in composition, as has been shown by the author in the case of the subnitrate of copper; for they all bear an analogy to this salt in their small solubility and other properties, while they exhibit little resemblance to those classes of phosphates and arseniates which really possess more than one atom of base. A table is then given, containing the formulæ expressing the composition of the most important phosphates, together with a new nomenclature by which, in accordance with his views, the author proposes to designate these salts. He then enters into the details of experiments illustrating the composition of, 1st, tribasic phosphate of soda, ammonia, and water, (or the microcosmic salt of the old chemists): 2ndly, tribasic phosphate of zinc and water, (or what is commonly called phosphate of zinc): 3rdly, tribasic arseniate of magnesia and water, (the common arseniate of magnesia): 4thly, tribasic phosphate of magnesia and water, (or ordinary phosphate of magnesia): and 5thly, tribasic phosphate of magnesia and ammonia, (or ammoniaco-magnesian phosphate).

In the fourth section he treats of sulphates, and supports, by further evidence, the opinion he formerly advanced; that as bisulphate of potash is a double sulphate of water and potash, and therefore neutral in its composition, so, with the sole exception of the anormal class already noticed, all salts, usually considered as bisalts are, in like manner, really neutral in composition. He shows that this theory is strictly applicable to the red chromate of potash, which appeared to present a difficulty.

The chlorides are next considered. The law followed by the chlorides of the magnesian class of metals appears to be that they have two atoms of water strongly attached to them, and which may therefore be regarded as constitutional. Thus, chloride of copper crystallizes with two atoms of water, and with no lower proportion; but several chlorides of this class have two or four atoms more; the proportion of water advancing by multiples of two atoms. The chlorides have probably their analogues in the cyanides, although we

are less acquainted with the single cyanides of iron, copper, &c.: but the disposition of the protocyanide of iron, and of the cyanide of copper to combine with two atoms of cyanide of potassium, may depend on the cyanides of iron and of copper possessing, like the corresponding chlorides, two atoms of constitutional water, which are displaced by two atoms of the alkaline cyanide in the formation of the double cyanides.

December 15, 1836.—A paper was read, entitled, "Further Observations on the Optical Phenomena of Crystals." By Henry Fox Talbot, Esq., F.R.S.

The author had described, in a former paper *, the remarkable circular mode of crystallization frequently occurring from a solution of borax in phosphoric acid, and producing, when examined by the polarizing microscope, the appearance of a black cross, with four sectors of light, and occasionally coloured rings, upon each crystal. In the present memoir, he describes some deviations from the usual forms of crystalline circles; the most striking varieties consisting in the cross being itself highly coloured, instead of black, upon a white ground. The author shows that these crystals consist of boracic acid alone, resulting from the decomposition of the borax by the phosphoric acid. He gives an explanation of the optical appearances they present on the hypothesis of their being constituted by an aggregate of acicular crystals, radiating from a central point, and the whole circle being of variable thickness at different distances from its centre, and acting with great energy on polarized light. Other modes of crystalline formation, dependent chiefly on the presence or absence of combined water, are next described. These sometimes produce crystals composed of two opposite sectors of a circle, united at the centre; at other times, they exhibit irregular elongated shapes, having a stem, either subdivided at both extremities into minute diverging fibres, or abruptly truncated; and occasionally they present regular geometric forms: but, whatever be their shape, they undergo, in general, spontaneous changes in the course of one or two days after they have been formed.

The author then notices a property belonging to some crystals, similar to that possessed by the tourmaline, of analysing polarized light; for which reason he denominates them *analytic crystals*. As an example, he mentions those obtained by dissolving sulphate of chromium and potash in tartaric acid by the aid of heat. A drop of this solution, placed on a plate of glass, soon yields, by evaporation, filmy crystals, which frequently have this property. The plumose crystals of boracic acid, when crystallized from a solution of borax in phosphoric acid, also possess this analytic power, and present very beautiful appearances when viewed with the polarizing microscope. Another instance occurs in the oxalate of potash and chromium, a salt whose optical properties have been investigated by Sir David Brewster. If gum arabic be added to a solution of this salt, and a drop of it be put between two plates of glass, a very beautiful arborescent, but microscopic crystallization takes place,

* See our last volume, p. 288.

composing a multitude of minute prisms, growing, as if by a species of vegetation, and variously arranged in sprigs and branchlets, often resembling in miniature, the tufts of marine confervæ. A similar plumose appearance, accompanied with the same analytic properties, is obtained from the evaporation of a drop of a mixed solution of nitre and gum arabic. This analytic effect is shown to be the consequence of the high degree of doubly refractive power possessed by these crystalline filaments, and which exists even in those whose diameter is evanescent on microscopic examination. The author entertains hopes that it will be possible to obtain large and permanent artificial crystals, which may possess the advantages of the tourmaline, without the inconvenience resulting from its dark colour.

December 22, 1836.—"First Memoir on the Theory of Analytical Operations." By the Rev. Robert Murphy, M.A., F.R.S., Fellow of Caius College, Cambridge.

The author considers the elements of which every distinct analytical process is composed, as of three kinds; the first, being the *subject*, that is, the symbol on which a certain notified operation is to be performed; the second, the *operation* itself, represented by its own symbol; and the third, the *result*, which may be connected with the former two by the algebraic symbol of equality. The operations are either *monomial* or *polynomial*; *simple* or *compound*; and with respect to their order, are either *fixed* or *free*. He uses the term *linear* operations to denote those of which the action on any subject is made up by the several actions on the parts, connected by the signs *plus* or *minus*, of which the subject is composed; and these linear operations likewise may be monomial or polynomial.

A variety of theorems for the development of functions of a very general nature are then deduced from expansions of the fundamental expressions, derived from the principles stated in the beginning of this memoir: and various laws embracing the relations subsisting between analytical operations, and the fundamental formulæ for their transformation are investigated.

"Observations and Experiments on the Solar Rays that occasion Heat; with the application of a remarkable property of these rays to the construction of the Solar and Oxy-hydrogen Gas Microscopes." By the Rev. J. B. Reade. Communicated by J. G. Children, Esq., Sec. R.S.*

The method employed by the author for obtaining, by a combination of lenses, the convergence to foci of the colorific solar rays, together with the dispersion of the calorific rays, consists in making a beam of solar light, which contains both kinds of rays, pass, after it has been converged to a focus by a convex condensing lens, through a second convex lens, placed at a certain distance beyond that focus: that distance being so adjusted as that the calorific rays, which, from their smaller refrangibility, are collected into a focus more remote from the first lens than the colorific rays, and consequently nearer to the second lens, shall, on emerging from the latter, be either parallel or divergent; while the colorific rays, which,

* See our present Number, p. 184.

being more refrangible, had been collected into a focus nearer to the first lens, and more distant from the second, will be rendered convergent by this second lens; so that the second focus, into which they are thus collected, will exhibit a brilliant light without manifesting any sensible degree of heat. The light so obtained may be advantageously applied to the solar, and to the oxy-hydrogen microscopes, from its producing no injurious effects on objects inclosed in Canada balsam, or even on living animalcules exposed to its influence.

Another improvement in the construction of the microscope employed by the author, consists in the cell for holding objects being made to move quite independently of the field glass; so that the best focus is obtained by an adjustment which does not disturb the field of view.

January 12, 1837.—"An attempt to account for the discrepancy between the actual Velocity of Sound in Air or Vapour, and that resulting from theory." By the Rev. William Ritchie, LL.D., F.R.S. Professor of Natural Philosophy at the Royal Institution, and in University College, London.

Sir Isaac Newton determined from theory that the velocity of the undulations of an elastic medium generally is equal to that which a heavy body acquires in falling by the action of gravity through half the height of a homogeneous atmosphere of that medium; but the actual velocity of sound in atmospheric air is found to be one eighth greater than what is assigned by that formula. This difference was attempted to be accounted for by Newton on the supposition that the molecules of air are solid spheres, and that sound is transmitted through them *instanter*. Laplace endeavoured to reconcile the difference between theory and observation, by the hypothesis that heat is disengaged from each successive portion of air during the progress of the condensed wave. The author of the present paper regards the hypothesis of Laplace as a gratuitous and improbable assumption; the falsehood of which he thinks is apparent from the fact that a rarefied wave advances through air with the same velocity as a condensed wave, which would not be the case if in either instance their progress were influenced by the heat evolved. He then enters into calculations to show that if the molecules of water be assumed as incompressible, and, when at the temperature of maximum density, very nearly in absolute contact, we ought, in estimating the velocity of sound in steam, to add to the velocity given by the formula of Newton, the rectilinear space occupied by the molecules; which, if a cubic inch of water be converted into a cubic foot of steam, will be one twelfth of the distance. By comparative experiments with a tuning-fork held over a tube, closed at one end, and containing at one time air, and at another steam, and also by similar trials with organ pipes of variable lengths, the author found a close agreement between his theory and observation. He also shows that this theory furnishes the means of determining, *à priori*, the density of a liquid, if the velocity of sound in the vapour of that liquid be given. In a postscript he adduces further confirmation of

the truth of his theory by observations on the velocity of sound n hydrogen gas, and in carbonic acid gas.

January 19.—"Researches towards establishing a Theory of the Dispersion of Light." By the Rev. Baden Powell, M.A., F.R.S., Savilian Professor of Geometry in the University of Oxford.

The author here prosecutes the inquiry on the dispersion of light which was the subject of his former papers published in the Philosophical Transactions for 1835 and 1836, extending it to media of higher dispersive powers, which afford a severer test of the accuracy of M. Cauchy's theory. He explains his methods of calculation and the formulæ on which his computations are founded, and which are different from those employed in his former investigations: and then states the results in a tabular form. On the whole he concludes that the formula, as already deduced from the undulatory theory, applies sufficiently well to the case of media whose dispersion is as high as that of oil of anise-seed: or below it, such as nitric, muriatic, and sulphuric acids, and the essential oils of angelica, cinnamon, and sassafras, balsam of Peru, and kreosote. It also represents, with a certain general approximation to the truth, the indices of some more highly dispersive bodies. The author therefore considers it as extremely probable that the essential principle of the theory has some real foundation in nature. From the regularity which he finds in the deviation of observation from theory, he thinks it likely that the formula only requires to receive some further development, or extension, in order to make it apply accurately to the higher cases, while it shall still include the simpler form which so well accords with the lower.

"A few remarks on the Helm Wind." By the Rev. William Walton, of Allenheads, near Hexham. Communicated by P. M. Roget, M.D., Sec. R.S.

On the western declivity of a range of mountains, extending from Brampton, in Cumberland, to Brough, in Westmoreland, a distance of 40 miles, a remarkably violent wind occasionally prevails, blowing with tremendous violence down the western slope of the mountain, extending two or three miles over the plain at the base, often overturning horses with carriages, and producing much damage, especially during the period when ripe corn is standing. It is accompanied by a loud noise, like the roaring of distant thunder; and is carefully avoided by travellers in that district, as being fraught with considerable danger. It is termed the *helm wind*; and its presence is indicated by a belt of clouds, denominated the *helm bar*, which rests in front of the mountain, three or four miles west of its summit, and apparently at an equal elevation, remaining immoveable during twenty-four or even thirty-six hours, and collecting or attracting to itself all the light clouds which approach it. As long as this bar continues unbroken, the wind blows with unceasing fury, not in gusts, like other storms, but with continued pressure. This wind extends only as far as the spot where the bar is vertical, or immediately over head; while at the distance of a mile further west, as well as to the east of the summit of the mountain, it is not unfre-

quently almost a perfect calm. The author details the particulars of an expedition which he made with a view to investigate the circumstances of this remarkable meteorological phenomenon, and proposes a theory for its explanation.

"A Meteorological Journal kept at Allenheads, 1400 feet above the level of the Sea, from the 1st of May to the 1st of November, 1836." By the Rev. William Walton. Communicated by P. M. Roget, M.D., Sec. R.S.

January 26.—A paper was read, "On the Structure of the Brain in Marsupial Animals." By Richard Owen, Esq., F.R.S., Hunterian Professor of Anatomy to the Royal College of Surgeons.

The author describes a remarkable modification in the commissural apparatus, apparently provided with a view to establish communications between the cerebral hemispheres, which he has observed in the brains of marsupial animals, and which has hitherto been regarded as constituting the essential difference between the brains of oviparous and mammiferous vertebrata, but which he considers as indicating a certain relation between the greater perfection of that organ, resulting from the superior magnitude of the great commissure, or corpus callosum, and the placental mode of development in the true mammalia. In a former paper he adduced evidence tending to show that both a small development of the cerebral organ, and an inferiority of intelligence are the circumstances in the habits and structure of this singular tribe of animals most constantly associated with the peculiarities of their generative economy: and the repeated dissections he has since made, an account of which is given in the present paper, have afforded him the most satisfactory confirmation of this coincidence, between a brief intra-uterine existence, together with the absence of a placental connexion between the mother and the fœtus, and an inferior degree of cerebral development. Thus, on comparing the structure of the brain in the Beaver and in the Wombat, he finds that the corpus callosum, or great commissure which unites the supraventricular masses of the hemispheres in the former, as well as in all other placentally developed mammalia, and which exists in addition to the fornix, or hippocampal commissure, is wholly absent in the latter animal: and that a similar deficiency exists in the brain of the Great and Bush Kangaroos, of the Vulpine Phalanger, of the Ursine and Mange's Dasyurus, and of the Virginian Opossum; whence he infers that it is probably the characteristic feature of the structure of the marsupial division of mammalia. In this modification of the commissural apparatus, the Marsupiata present a structure of brain which is intermediate between that of the Placental Mammalia and Birds; and hence the Marsupiata, together with the Monotremata, may be regarded as constituting a distinct and peculiar group in the former of these classes, although they include forms, which typify the different orders of the ordinary Mammalia.

LINNÆAN SOCIETY.

Dec. 20, 1836.—A paper was read, entitled "Descriptions of the Species of *Polygonum* and *Fagopyrum* contained in the Indian Herbarium of Professor Royle, F.L.S., Sec. Geol. Soc. By Charles C. Babington, Esq., M.A., F.L.S.

Professor Royle's collections were chiefly formed in Sirmore, Kunawar, and Cashmere, and comprise a considerable number of new species, besides many that are identical with those found by Dr. Wallich in Nepal. The most interesting additions to the present genus consist of five species of the section *Avicularia*. We subjoin an enumeration of the species, together with characters of the new ones.

POLYGONUM, *Linn.*

Sect. 1. Bistorta, *Meisn.*

1. P. bulbiferum.

Spicâ compactâ densâ basi interruptâ laxiusculâ, bracteis ovatis acuminatis subincisis, staminibus calyce brevioribus filamentis post anthesin elongatis, stylis 2 rariùs 3 calyce duplò longioribus, achenio calycis longitudine lenticulari faciebus rotundato-acuminatis minutè granulato-striatis subopacis, foliis caulinis subsessilibus lanceolatis radicalibus petiolatis ellipticis vel elliptico-lanceolatis omnibus in margine revoluto costato-crenatis.

P. bulbiferum, Royle MSS.

2. P. macrophyllum, *Don.*
3. P. amplexicaule, *Don.*
4. P. vaccinifolium, *Meisn.*
5. P. affine, *Don.*
6. P. Emodi, *Meisn.*

Sect. 2. Amblygonon, *Meisn.*

7. P. orientale, *Linn.*

Sect. 3. Persicaria, *Meisn.*

8. P. lanigerum, *R. Br.*
9. P. hispidum, *Don.*
10. P. barbatum, *Linn.*
11. P. scabrinervium.

Spicis pedunculatis geminatis subcymosis strictis laxis pedunculis longioribus, bracteis acutis eglandulosis glabris 3—4-floris pedicellis subæqualibus, floribus 6-andris semidigynis, calyce 4-fido eglanduloso, staminibus inclusis, achenio lenticulari lævi nitido, pedunculis glandulosis, ochreis glabris non ciliatis, foliis lanceolatis glandulis flavis numerosissimis suprà et subtùs notatis glabris margine costâquè scabroso-pilosis, caule erecto ramoso in superiori parte glanduloso.

P. scabrinervium, Royle MSS.

12. P. simlense.

Spicis paniculatis pedunculatis ovato-oblongis multifloris, pedunculis glandulosis, bracteis turbinato infundibuliformibus parvis 2—3-floris pedicellis æqualibus, floribus 4-fidis, 6-andris semi-2-gynis, achenii lenticulari calyce absconditi faciebus planis nitidis minutissimè granulatis, ochreis cylindricis muticis glabris, foliis lanceolatis glabris costâ setoso-scabrâ exceptâ, margine scabro-ciliatis,

caule erecto subsimplici fistuloso glabro in superiori parte glanduloso.

P. simlense, Royle MSS.

13. P. glabrum, *Willd.* | 14. P. Donii, *Meisn.*

Sect. 4. Cephalophilon, *Meisn.*

Subsect. 1. Didymocephalon, *Meisn.*

15. P. filicaule, *Wall.*
16. P. punctatum, *Don.*
17. P. nepalense, *Meisn.*
18. P. sphærocephalum, *Wall.*
19. P. capitatum, *Don.*
20. P. sinuatum.

Capitulis solitariis, pedunculis glabris, bracteis ovatis obtusis, floribus 5-andris semitrigynis laciniis obtusis, achenio triquetro, ochreis glabris vel parcè pilosis, foliis lyratis lobo terminali rhomboideo, petiolo basi biauriculato, caule ramoso.

P. sinuatum, Royle MSS.

Subsect. 2. Corymbocephalon, *Meisn.*

21. P. chinense, *Linn.*

Sect. 5. Aconogonon, *Meisn.*

22. P. tortuosum, *Don.* | 23. P. Hagei.

Paniculi ramis subsimplicibus aphyllis, bracteis 3 – 6-floris pedicellos erectos subæquantibus, calycis laciniis rotundato-ovalibus obtusis glabris 2 exterioribus angustioribus, ochreis petiolo longioribus internodio brevioribus, foliis lanceolatis apice lineari-attenuatis subtùs lanato-velutinis suprà pubescentibus, caule erecto ramoso striato cum ramis pedunculis ochreisque pilosis.

P. Hagei, Royle MSS.

24. P. polystachyum, *Wall.* | 25. P. rumicifolium.

Paniculis subsimplicibus, bracteis basi pilosis unifloris pedicellis brevioribus, calycis laciniis obovatis obtusis æqualibus, ochreis internodio dimidio brevioribus petiolo longioribus pilosis, foliis cordatis ovatisve pilosis, margine undulato, caule erecto striato.

P. rumicifolium, Royle MSS.

Sect. 6. Tinaria, *Meisn.*

26. P. Convolvulus, *Linn.* | 27. P. heterocarpum, *Wall.*

Sect. 7. Avicularia, *Meisn.*

28. P. herniarioides, *Delile.*
29. P. aviculare, *Linn.?*
30. P. Royle .

Floribus axillaribus parvis pedicello brevissimo, achenio trigono granulato-striato perigonio æquali, ochreis acutis lobatis haud laceris : venis 2 obsoletis, foliis lineari-lanceolatis acutis integris punctis glandulosis numerosissimis, caule 3—4-angulato, angulis scabris.

31. P. mucronatum.

Floribus axillaribus parvis sessilibus, achenio trigono lævi, ochreis ovatis acutis laceris internodiis longioribus nervis nullis, foliis lanceo-

lato-linearibus longè mucronatis marginibus recurvis, caule pubescenti lignoso.

P. mucronatum, Royle MSS.

32. P. recumbens.

Floribus axillaribus parvis pedicello brevi, achenio trigono lævi nitido angulis rotundatis segmentis carinatis perigonii clausi tecto, ochreis lanceolatis acutis demùm laceris: nervis 2 excurrentibus foliis ovatis brevi-petiolatis margine nervisque subtùs scabris.

P. recumbens, Royle MSS.

33. P. confertum.

Floribus axillaribus parvis globosis, pedicello brevi, achenio compresso trigonove lævi nitido segmentis non-carinatis perigonii campanulati tecto, ochreis lanceolatis acutis demùm laceris: nervis abbreviatis foliis oblongis 1-nervosis brevi-petiolatis margine nervoque subtùs scabris.

P. confertum, Royle MSS.

FAGOPYRUM, *Gærtn.*

1. F. rotundatum.

Floribus parvis paniculato-racemosis, achenio trigono angulis rotundatis in superiori parte carinatis calyce 4—5-plò longiore faciebus oblongo-ovatis rugosis, foliis triangulari-hastatis paulò longioribus quàm latis petiolatis, caule erecto annuo.

2. F. esculentum, *Mœnch.* | 3. F. emarginatum.

Floribus paniculatis parvis pedicello elongato, achenio trigono angulis alatis integris calyce obtuso duplò longiore faciebus ovatis longioribus quàm latis, foliis petiolatis triangularibus acutis, angulis inferioribus rotundatis.

P. emarginatum, Roth? Cat. Bot. I. 48. Don? Prod. 73. Meisn.? Mon. 62.

4. F. cymosum, *Meisn.*

Jan. 17, 1837.—Read the commencement of a paper by John O. Westwood, Esq., F.L.S., entitled "Illustrations of the Relationships existing amongst natural Objects termed Affinity and Analogy, selected from the class of Insects."

Feb. 7.—Read a notice, accompanied by specimens, of the discovery of *Polygonum dumetorum* and *Epipactis purpurata* in the vicinity of Reigate, Surrey. By Mr. George Luxford, A.L.S.

The chief distinctions of *Polygonum dumetorum* consist in its perfectly cylindrical stem, elongated pedicels, and smooth and shining fruit. The *P. Convolvulus* varies in the breadth of the margins of its sepals, and in some states it equals that of the former species. The racemes and pedicels in the latter are always much shorter, the stems angular, and the fruit opake, and beset with minute elevated dots.

The *Epipactis purpurata*, first described in the fourth volume of the "English Flora," appears to be only a variety of *E. latifolia*.

Specimens of a remarkable variety of *Pinus Pumilio*, having the scales of the cones singularly lengthened and reflexed, were exhi-

bited from the extensive collection of His Grace the Duke of Bedford at Woburn Abbey.

Mr. Lambert, V.P., exhibited specimens of the *Tamarix mannifera,* a species nearly related to *T. gallica,* and of the sweet gummy substance which exudes from the wounds occasioned by a species of *Coccus,* said by Ehrenberg and Hemprich to be peculiar to the valley at the foot of Mount Sinai, where the substance is collected, which is called "*Man*" by the Arabs, and supposed to be identical with the manna recorded in Scripture. The specimens were collected by Lieutenant Wellsted.

Read a continuation of Mr. Westwood's paper on Affinity and Analogy.

Feb. 21.—Read, Some Observations on the Manna of Mount Sinai, and the Dragon's Blood Tree and Aloe Plant of Socotra. By Lieut. Wellsted.

It is in Wady Hebron that the Manna is obtained by the Bedouins, who collect it early in the morning, and after straining it through cloths they put it into skins or gourds. The quantity collected in the most favourable seasons does not exceed 700 pounds. A considerable quantity is consumed by the Bedouins themselves, but a portion is sent to Cairo, and some is disposed of to the monks of the convent at Mount Sinai, who retail it to the Russian pilgrims, by whom it is received with much reverence as an incontestible proof of the truth of the event recorded in Scripture. The substance is only collected in seasons after heavy rains, for it has been known to be wanting for a period of seven years. When recent it has the consistence and flavour of honey, and is of a deep amber colour.

The Dragon's Blood Tree of Socotra appears to be identical with that of the Canary Islands, which is the *Dracæna Draco* of Linnæus. In Socotra it is rarely met with below the altitude of 800 feet, and it is frequently seen growing on the granite peaks at an elevation of 4000 or 5000 feet above the level of the sea. The gum exudes spontaneously, or from artificial incisions in the trunk. The season most favourable for obtaining it is in June, immediately after the setting of the S.W. monsoon.

The island of Socotra has been famous from the earliest period for its Aloes; but that article of export has of late years fallen into neglect, so that not more than two tons were exported in 1833. The plant abounds all over the island, and is most probably identical with the *Aloë officinalis* of Forskal, *Fl. Ægypt. Arab.* cent. 3. p. 73. The leaves are short and stained with a reddish-brown colour, and the flowers are red. The species belongs to the same group of the genus with the *Aloë vulgaris.*

Mr. Iliff, F.L.S., exhibited a portion of the trunk of an oak which was blown down in Windsor Park during the late hurricane, which upon being split was found to contain the letters W. B. and the year 1670 carved on it.

Read, the commencement of a paper by Joseph Woods, Esq., F.L.S., entitled "Observations on the European Genera of Grasses."

ROYAL ASTRONOMICAL SOCIETY.

Nov. 11, 1836.—The following communications were read:—

I. Extract from a Letter from Mr. Maclear to Captain Beaufort, accompanied by the original Circle and Transit Observations of Halley's Comet since January.

The number of meridian observations thus obtained is upwards of thirty. The reductions will be forwarded in a short time; the delay arising from Mr. Maclear being employed in observing the stars of the Brisbane list, in aid of Sir John Herschel, who, he states, is now occupied in reducing his Catalogue of Southern Nebulæ.

II. A Catalogue of the Right Ascensions of 1318 Stars, observed at Blackheath. By Mr. Wrottesley.

These papers consist of a catalogue in Right Ascension of 1831 stars (those of the 6th and 7th magnitude inclusive, contained in the Astronomical Society's Catalogue), with an explanatory introduction and notes; and also of the original observations, and the reduced mean places, from which the catalogue is formed. As this is the first contribution by a private observer to a more accurate knowledge of the places of the fixed stars, which has been made in consequence of the Society's Catalogue, and according to its directions; and as the deductions are of great value and importance; a distinct report has been made by a Committee, and adopted by the Council, in order that due credit may be given to the labours of Mr. Wrottesley and of his assistant Mr. Hartnup.

The observations were made with a transit telescope, by Mr. Thomas Jones, of $3\frac{3}{4}$ inches clear aperture, 62 inches focal length, and 27 inches horizontal axis. The power used was 142. The position of the instrument was ascertained, when practicable, by consecutive transits of Polaris above and below pole: and in other circumstances, by single transits of Polaris or δ Ursæ Minoris. This was checked, in some degree, by a close mark seen through a fixed lens; which, however, discharged a better purpose in determining the error of collimation whenever the instrument was reversed. This was done every month. The level was always applied (of course in reversed positions) once every night, and often twice, viz. at the beginning and end of a series of observations. At first, corrections for the instrumental errors were computed and applied, but the amount was found to be so small, that Mr. Wrottesley subsequently preferred taking the clock error for the stars of his catalogue from the standard or standards, which, having nearly the same declination, were affected by the same instrumental errors. Thus the instrumental errors, which were always noted and kept low, were, as to sense, eliminated.

The bases of this catalogue are, the fundamental catalogue of Bessel for the mean places of the standard stars (omitting some not suited to Mr. Wrottesley's purposes, and substituting his own place of Fomalhaut for the erroneous place of Bessel;) and for the corrections, the constants and precessions of the Astronomical Society's catalogue, and the values of A, B, C, D, contained in the *Nautical Al-*

manac. The number of observations of the stars in the catalogue is 12007, or rather more than 9 observations for each star, on an average.

The partial mean places appear to be as close to each other, or nearly so, as those of the Greenwich observations. It follows, from Mr. Wrottesley's method of deducing the clock error for each star from one, two, or more standards which are near it, that the accidental errors are greater than he would have had, if he had used all the standard stars for his clock error; but, as it is, a greatest difference from the mean of ten or twelve observations exceeding $0^{s}{\cdot}3$ is not common, which leaves only a small uncertainty upon the final result; especially since this difference, being the sum of two independent errors of observation, viz. that of the standard determining star and that of the catalogue star, may be expected to have little effect in the mean of several observations. Indeed, there can be no doubt, that this catalogue of Mr. Wrottesley's may be used for all purposes, with nearly, if not altogether, the same confidence as the fundamental catalogue from which it is derived.

Of the means taken to insure accuracy in the reductions, Mr. Wrottesley has given a very satisfactory account, and that these means have been effectual, he has stated the following proof. In Mr. Wrottesley's catalogue are 138 stars observed and reduced by Professor Airy in the Cambridge observations. "Of these, 46 agree within $0^{s}{\cdot}05$; 89 within $0^{s}{\cdot}10$; 115 within $0^{s}{\cdot}15$; 131 within $0^{s}{\cdot}20$; and only one differs so much as $0^{s}{\cdot}30$." We have a further and independent proof of the correctness of this catalogue in the Remarks (appended by Mr. Baily) on the differences of $1^{s}{\cdot}0$ and upwards, between the catalogue of Mr. Wrottesley and that of the Astronomical Society. It is known to the Society, that our catalogue has been pretty nearly reobserved by Mr. Taylor at Madras, and very well observed, though, unfortunately, very few copies [of Mr. Taylor's catalogue] have found their way into the hands of English astronomers. There are 29 stars having such differences, of which 24 have been observed by Mr. Taylor, and in every instance his result confirms that of Mr. Wrottesley. Whether these anomalies are to be attributed to errors in the catalogues of Bradley or Piazzi (for Mr. Baily has examined the computations by which our catalogue is deduced from theirs), or whether there is some irregular motion in the stars themselves, time will show. It is from such undertakings as this of Mr. Wrottesley, and so executed, that we must expect to fix the state of the heavens at certain epochs, and so prepare the data for future speculation and future discovery. For such inquiries, Mr. Wrottesley's present of the original transit books, to which the partial mean places serve as a complete index, will be of great and permanent value.

There are several remarks which deserve, and doubtless will receive, the attention of practical astronomers, but which would be here out of place. We must not, however, omit mentioning that the whole work has been performed, to use Mr. Wrottesley's words, "without any foreign aid," by himself, and, under his superintendence, by his assistant Mr. John Hartnup; upon whom, indeed, in

consequence of Mr. Wrottesley's frequent and continued absence from home, the task of observing and computing chiefly fell, and who has executed this task with extraordinary zeal, skill, and fidelity.

III. On the Projection of Maps and Charts. By Professor Littrow.

Three kinds of projections are chiefly had recourse to in the construction of maps,—the orthographic, the stereographic, and the central. The object of Professor Littrow is to deduce the general properties of these three principal projections, which, though they differ from each other in no other respect than in the situation of the eye and perspective plane, with regard to the principal circles of the sphere, have hitherto been always treated as distinct and independent problems.

The concluding part of the memoir contains some general remarks on the solutions of the general problem by Gauss and Lagrange; and a demonstration that Gauss's formulæ (contained in a memoir a translation of which appeared in Phil. Mag. and Annals, N.S., for August and September 1828,) are comprehended in those of Lagrange, the latter being only particular values of the former.

IV. On the construction of the Hour-lines of Sun Dials. By Professor Littrow.

V. On the Formulæ for the computation of Precession. By M. Mattheus Valente do Conto, Director of the Observatory at Lisbon.

VI. Notice of a forthcoming work on the Measures of Double Stars. By Professor Struve.

Professor Struve hoped, in June last, to complete his extended catalogue of double stars, containing all the observations made since those already published in his well-known *Catalogus Stellarum Duplicium*, Dorpat, 1827 (or from 1824 to 1836). This last-mentioned catalogue contained 3112 stars; from which the professor has, for various reasons assigned, excluded 490, and has added 64 remarkable new ones of greater distance than 32″, and 21 of less distance. The number of stars, therefore, is 2707. The measures were made with a wire-micrometer applied to the large refractor, with a power varying from 320 to 1000, and mostly in an illuminated field. Calling each night's observations of one star a measure, the number of measures is about 11,000, or, on an average, four to each star.

Professor Struve has divided these stars into eight classes (Sir W. Herschel used four), as follows:

W. Herschel.	Struve.
I. 0″ to 4″ of distance.	I. 0″ to 1″ of distance.
	II. 1 — 2 ——
	III. 2 — 4 ——
II. 4 — 8 ——	IV. 4 — 8 ——
	V. 8 — 12 ——
III. 8 — 16 ——	VI. 12 — 16 ——
	VII. 16 — 24 ——
IV. 16 — 32 ——	VIII. 24 — 32 ——

Each class is further divided into two divisions, *lucidæ* and *reliquæ*; the former containing those in which the companion is not of less than the eighth magnitude. The principle of this division is, Professor Struve states, that the catalogue is very nearly complete with respect to the *lucidæ*, on which, therefore, certain theoretical conjectures may be formed, relative to the numbers of double stars in different orders of magnitude. The introduction, besides all the matters explanatory of the present and emendatory of the former, catalogue, which might certainly have been looked for from Professor Struve, will contain conclusions concerning the nature of double stars, from their distribution among the orders of magnitude, their brightness, proper and relative motions, &c. Professor Struve has added a specimen of the catalogue, and several interesting conclusions, of which our limits will only enable us here to notice the very rapid motion of 42 Comæ Berenices (130° in six years), the reduction of the period of λ Ophiuchi to less than 40 years, and the close approach to their nearest distance of γ Coronæ and ω Leonis. The latter system, between Sir William Herschel and Professor Struve, has now been watched from the greatest to the least apparent distance.

VII. Stars observed with the moon at the Royal Observatories of Greenwich and Edinburgh, and the Observatory of Cambridge, in the months of June—October, 1836.

Dec. 9, 1836.—The following communications were read, viz.:—

I. On a remarkable phænomenon that occurs in total and annular eclipses of the sun. By Mr. Baily, Vice-President of the Society.

The author states, that, having read of certain singular appearances that are recorded as having taken place in annular eclipses of the sun, at the moment that the whole disc of the moon enters on the disc of the sun, he was desirous of witnessing those phænomena at the solar eclipse of May 15th last; and, finding that the central path of the moon's shadow would pass nearly in a straight line from Ayr, on the western coast of Scotland, to Alnwick on the eastern coast of Northumberland, he proceeded to Scotland for that express purpose. Having computed, from the elements given in the Nautical Almanac, that the central line of the moon's umbra would pass directly over, or very near to, Jedburgh in Roxburghshire; and having ascertained that this place was within eight or ten miles of Makerston, the seat of Lieut.-General Sir Thomas Macdougal Brisbane, Bart., who has a well-furnished observatory there, and from whom he was sure of obtaining the correct time for his chronometers, he resolved to make that town his head-quarters. Mr. Baily took with him a $3\frac{1}{2}$-feet refracting telescope by Dollond, $2\frac{5}{8}$ inches aperture, and magnifying about 40 times; a 20-inch Rochon's prismatic telescope, for measuring the distances between the borders of the sun and moon; two thermometers; a burning-glass; and four pocket chronometers.

Mr. Baily took up his station at the house of Mr. Veitch, a very ingenious gentleman, residing at Inch Bonney, about half-a-mile to

the southward of the town of Jedburgh, who afforded him every facility for making the observations. The morning of the 15th of May is described as being remarkably fine and clear; not a cloud to be seen in any part of the heavens during the whole time of the eclipse. The times of the beginning and ending of the eclipse, and of the formation and dissolution of the annulus, have already been given in the third volume of the monthly abstracts of the Society's proceedings, page 200. But Mr. Baily does not lay much stress on this part of his observations—more especially those connected with the annulus—since his attention was taken up with other more interesting phænomena. He says he was in expectation of meeting with something extraordinary at the formation of the annulus; but imagined that it would be only momentary, and consequently, that it would not interrupt the noting of the time of its occurrence. In this, however, he was deceived, as the following facts will show. For when the cusps of the sun were about 40° asunder, a row of lucid points, like a string of beads, irregular in size and distance from each other, *suddenly* formed round that part of the circumference of the moon that was about to enter on the sun's disc. This he intended to note as the correct time of the formation of the annulus, expecting every moment to see the thread of light completed round the moon; and attributing this serrated appearance of the moon's limb (as others had done before him) to the lunar mountains; although the remaining portion of the moon's circumference was perfectly smooth and circular, as seen through his telescope. He was somewhat surprised, however, to find that these luminous points, as well as the dark intervening spaces, increased in magnitude; some of the contiguous ones appearing to run into each other like drops of water. Finally, as the moon pursued her course, these dark intervening spaces were stretched out into long, black, thick, parallel lines, joining the limbs of the sun and moon: when all at once they *suddenly* gave way, and left the circumferences of the sun and moon in those points, as in all the rest, apparently smooth and circular, and the moon perceptibly advanced on the face of the sun. This moment of time Mr. Baily considers to be that which most persons would assume and record as the formation of the annulus; but he adduces strong reasons afterwards to show that the true formation of the annulus was some seconds prior to that event.

After the formation of the annulus, as thus described, the moon preserved her circular outline during its progress across the sun's disc, till her opposite limb again approached the border of the sun, and the annulus was about to be dissolved. When, all at once (the limb of the moon being at some distance from the edge of the sun), a number of long, black, thick, parallel lines, exactly similar in appearance to the former ones above mentioned, *suddenly darted forward*, and joined the two limbs as before; and the same phænomena were repeated, but in an inverse order. For, as those dark lines got shorter, the intervening bright parts assumed a more circular shape, and at length terminated in a fine, curved line of bright

beads (as at the commencement), till they ultimately vanished, and the annulus consequently became wholly dissolved. This remarkable and singular phænomenon was also observed by Mr. Veitch, and by Sir Thomas Brisbane, as well as by Mr. Henderson at Edinburgh; with some slight differences, however, in the detail. The appearance of the dark lines, or threads, was likewise noticed by Mr. Bell, at Alnwick, who sent an account of the same to the Philosophical and Literary Society at Newcastle. Mr. Baily describes them to have been as plain, as distinct, and as well defined, as the open fingers of the human hand held up to the light; and that there could not have been any doubt as to their form and existence, since they were seen by different observers, at different places, and with different telescopes. Several drawings accompanied the paper, showing the appearances at various stages of the annulus.

The number of these dark lines, or threads, Mr. Baily considers to have been about eight: in which opinion he was confirmed by Mr. Veitch. Sir Thomas Brisbane, however, thinks there were not more than six; whilst Mr. Bell, who noticed four at the dissolution of the annulus, says that there were only two at its formation. On these and other points Mr. Baily thinks there is ample room for a diversity of opinion, since the observer is taken, as it were, by surprise, and the phænomenon itself, during the short period of its existence, is constantly varying in some minute particulars.

Mr. Baily remarks, that the diminution of light was not so great during the existence of the annulus as was generally expected, being little more than might be caused by a temporary cloud passing over the sun: the light, however, was of a peculiar kind, somewhat resembling that produced by the sun shining through a morning mist. The thermometer in the shade fell only about three or four degrees. The birds in the hedges were in full song during the whole time of the eclipse. About twenty minutes before the formation of the annulus, Venus was seen with the naked eye; and a few minutes afterwards it was impossible to fire gunpowder, with the concentrated rays of the sun, through a lens of three inches in diameter. The same lens, likewise, had no effect on the ball of a thermometer during the existence of the annulus.

For the cause of the remarkable optical deception above described, Mr. Baily does not attempt to account; but he confesses his surprise that the phænomenon has not (with one single exception, which will be presently alluded to) been noticed or recorded, on former occasions, since it must have been seen by every person who watched for the formation and dissolution of the annulus; and although detached portions of the phænomenon have been recorded by different observers, as seen at different places (various extracts from whose accounts are quoted by Mr. Baily), yet it is impossible from those descriptions to form an accurate idea of the whole, or to trace the origin, progress, and termination of this phænomenon, which is certainly one of the most remarkable in astronomy. M. Van Swinden is the only person who has placed on record the ob-

servation of the dark lines, or threads, which connect the borders of the sun and moon, at the formation and dissolution of the annulus. His account is inserted in the first volume of the Memoirs of this Society (page 146), accompanied with drawings, which coincide almost exactly with those given by Mr. Baily. In nearly all the accounts by other observers, the description of the phænomenon is restricted to the very commencement of the annulus, or to the formation of the string of luminous points which on a sudden are seen to surround that portion of the moon's limb about to enter on the sun's disc; and no notice whatever is taken of the continuation of the phænomenon, or of the stretching out of the dark spaces into parallel lines, as above mentioned: nor of their *sudden* rupture and *disappearance,* which is by far the most remarkable part of the phænomenon.

How far any of these appearances may favour the hypothesis of a lunar atmosphere, or whether, indeed, they could be accounted for on such an assumption, the author does not stop to discuss; but, with a view to assist those who are disposed to enter on such an inquiry, he has adduced various accounts of a similar phænomenon to that of the dark lines, observed at the transits of Venus over the sun in 1761 and 1769. For on each of those occasions, many astronomers remarked, that, at the interior contact of Venus with the sun (both on its ingress and egress), there was formed a sort of dark ligament between the border of Venus and the border of the sun, which appeared like a protuberance from the planet, and which continued several seconds. This dark ligament is represented, in the drawings which accompany the several memoirs on this subject, to be much thicker, and to continue longer, than the dark lines in a solar eclipse; so that the planet, during the progress of the ingress and egress, assumes a shape which has been variously described as resembling a pear, a Florence flask, and a skittle. But all the accounts agree in stating the *sudden* rupture of the ligament, and that immediately thereon the planet assumes its usual circular shape. Nothing of this kind, however, has been noticed at the transits of Mercury over the sun: on the contrary, we have the direct evidence of Sir William Herschel (who examined Mercury, with that special object in view, at the transit of November 9, 1802), that he could not discern anything out of the usual course. He expressly states, that the whole disc of Mercury was as sharply defined as possible; and that there was no kind of distortion of the limb, either at its ingress or egress: the appearance of the planet remained well defined from first to last.

Mr. Baily considers, and adduces certain facts to show, that the circular edge of the moon is always distorted at those points which are in contact (or nearly so) with the sun's circumference; and which have occasionally given rise to the supposition of lunar mountains in high relief*. He thence infers, that all measures of the moon's diameter, when passing over the sun's disc, must be taken with great caution, and with due attention to the proximity of the

[* See Lond. and Edinb. Phil. Mag., vol. ix. p. 73.—Edit.]

part measured to the edge of the sun's disc (where alone the distortion seems to take place), otherwise errors and discordances will occur. Those prodigious lunar elevations and depressions, so frequently described in solar eclipses, are seldom or never seen, except at the commencement or termination of the eclipse, or in places near the solar cusps: that is, in those points only which are near the edge of the sun; every other portion of the moon's circumference being comparatively smooth and circular. If this notion be correct, it would seem that the measurement of the solar cusps during an eclipse may be liable also to discordances from this very cause.

Mr. Baily concludes, by expressing a hope, that, at the total eclipse of the sun in 1842, and the annular one in 1847 (both of which will be central in Europe), the attention of astronomers will be directed more particularly to this subject, both as to its existence and its cause; and that such a regular system of observations in various places will be adopted, as may best tend to elucidate and explain this very remarkable phænomenon.

There was laid on the table, for the inspection of the members present, a small floating collimator, made by M. Amici. This instrument was only $1\frac{1}{2}$ inch in length, and, together with the mercury on which it floats, was packed in a small round box, 2 inches diameter in the inside, and 2 inches high, which might be carried in the pocket. It is intended for voyagers, and other persons, to whom a larger instrument would be a great inconvenience. It was the first that had ever been made of such small dimensions.

There was also laid on the table a drawing, or representation, of several *shooting stars*, that were observed at Plymouth from the 11th to the 14th of November last, together with the direction which they severally took, as compared with the fixed stars then visible.

II. Stars observed with the moon at the Royal Observatories of Greenwich and Edinburgh, and the Observatory of Cambridge, in the month of November, 1836.

XLVII. *Intelligence and Miscellaneous Articles.*

ON THE SYMMETRIZING POWER OF THE EYE. BY THE REV. J. G. MACVICAR, A.M.

To the Editors of the Phil. Mag. and Journal of Science.

GENTLEMEN,

THE many interesting communications which have appeared in your Journal of late years on the subject of vision induce me to send an account of the following experiment, in the hope that it will not be unacceptable.

Let the surface of a glass mirror be sprinkled over with some powder, as, for instance, with flour from a dredging-box. This

done, on looking perpendicularly down upon the reflecting surface, at the distance of distinct vision from it (unless the eye be too long-sighted), the powder will appear, not irregularly scattered, as it really is, but symmetrically distributed in two systems of beautiful radiations, having the pupils of the eyes for their centres.

The phænomenon is sufficiently remarkable to strike even those who are not otherwise curious in such matters. It may be observed, however, that as every eye cannot catch it at once, it is better to commence by using one eye only, as this gives only one system of radiations, which, being more simple, is more easily observed. If this phænomenon has not been already attended to (and I do not recollect to have seen it noticed anywhere), it is, I think, well worthy of investigation. Some facts are, indeed, immediately obvious respecting it. Thus, as to the region in which the physical part of the phænomenon takes place, it plainly appears that it is not either the humours or retina, as is generally supposed in reference to other phænomena of the same order, but a more deeply seated part of the apparatus of vision. For if it were any of the anterior parts, or even the retina itself, the centre of the radiant system would certainly change its place when the eye was made to wander over the mirror*. In point of fact, however, that centre does not change place except when the whole head is moved, in which case it does so proportionally.

I ascribe the phænomenon to a peculiar mode of action in the nervous part of the apparatus of vision proper to it as an elastic tissue, in virtue of which it tends, like the tissues and media experimented on by Chladni, Savart, Faraday, and others, and doubtless all elastic tissues and media, to distribute all motions impressed upon it in symmetrical systems; a view of the matter having very interesting bearings upon the principles of taste,—during the investigation of which it was that this experiment first occurred to me,—and one calculated to explain several seemingly unaccountable phænomena as to the distribution of sensibility in the retina.

Johnfield by Dundee, Oct. 14.

STARCH.

M. Payen, in a memoir on starch, considers that this substance, in whatever manner or from whatever part of vegetables it may be obtained, whatever may be its form, its age, or its state of aggregation, has always the same chemical constitution: its conversion into dextrine by diastase, sulphuric acid, potash, &c., are modifications of its physical properties, without in the least degree altering its chemical constitution, which is represented by $C^2\ H^5\ O^5$.—*Jour. de Pharmacie,* Oct., 1836.

ON THE ACTION OF SULPHUROUS ACID ON STEEL.

The experiments which M. Vögel has made on this subject lead to the following results:

*[We do not feel certain that this would be the case, if the seat of the symmetrizing power be in the retina.—Edit.]

1st. A quantity of hydrosulphuric acid gas is formed during the action of sulphurous acid upon steel, which is not disengaged, but is decomposed as soon as formed, by the sulphurous acid, which causes the separation of sulphur.

2nd. Liquid sulphurous acid, which has been digested for a sufficient time upon steel, contains, besides the sulphite, a portion of hyposulphite of protoxide of iron, and this solution, when neutral, partially reduces the proto- and per-salts of mercury.

3rd. Concentrated liquid sulphurous acid, digested in close vessels with excess of steel, forms small crystals of a greenish white colour, which are insoluble in water, and act like a hyposulphite of iron with excess of base.

4th. The residuum which is left when steel is treated in close vessels with a sufficient quantity of sulphurous acid, is not pure carbon, but consists of carbon mixed, besides sulphur, with a basic hyposulphite of iron, which is difficultly soluble in sulphurous acid, and which shows that that acid is unfitted for analysing steel or iron.—*L'Institut.*

ANALYSES AND CHARACTERS OF MINERALS, BY M. KUDERNATSCH AND COUNT SCHAFFGOTSCH.

The analyses of which the results are given below were performed in the private laboratory of Prof. H. Rose.

Tin pyrites. Analysed by M. Jos. Kudernatsch. (From Poggendorff's *Annalen*, Band xxxix. Stück 9.)

Sulphur	29·64
Tin	25·55
Copper	29·93
Iron	12·44
Zinc	1·77
Earthy matter	1·02
	99·81

The composition of this mineral may be represented by the formula

$$\left.\begin{matrix}\mathrm{Fe}'^{2}\\ \mathrm{Zn}^{2}\end{matrix}\right\}\mathrm{Sn}'' + \underline{\mathrm{Cu}}''^{2}\,\mathrm{Sn}''.$$

Tennantite. Analysed by M. Jos. Kudernatsch. (From Poggendorff's *Annalen*, Band xxxviii. Stück 2.)

Sulphur	27·76
Arsenic	19·10
Copper	48·94
Iron	3·57
Silver	a trace
Quartz	0·08
	99·45

The probable formula for tennantite is

$$\left.\begin{matrix}\mathrm{Fe}'^{4}\\ \mathrm{Cu}'^{4}\end{matrix}\right\}\underline{\mathrm{As}}'' + 2\,\underline{\mathrm{Cu}}'^{4}\,\underline{\mathrm{As}}'''.$$

Jamesonite from Estremadura. Analysed by Count F. Schaffgotsch. (From Poggendorff's *Annalen*, Band xxxviii. Stück 2.)

The specimens analysed were readily cleavable in a direction perpendicular to the axes of the crystals, and with some difficulty in several directions parallel to the axes of the crystals. Lustre, metallic. Colour, dark lead-gray. Streak, blackish gray. Hardness, a little greater than that of rock salt. Specific gravity = 5·616 at 19° cent.

Lead	39·971
Antimony	32·616
Sulphur	21·785
Iron	3·627
Bismuth	1·055
Zinc	0·421
	99·475

The following analyses of Augite, Amphibole, &c., by M. Jos. Kudernatsch, are from Poggendorff, Band xxxvii. Stück 4.

Augite from Zigolon-Berg in Fassathal. Specific gravity = 3·358 at 17° R.

	1.	2.	Oxygen in 2.
Silica	50·09	50·15	26·05
Alumina	4·39	4·02	1·87
Lime	20·53	19·57	5·49
Magnesia	13·93	13·48	5·21
Oxide of iron	11·16	12·04	2·74
	100·10	99·26	

Augite from Gillenfelder Maar in the Eifel. Specific gravity = 3·356 at 17° R.

	1.	2.	3.	4.	Of Oxygen in 4.
Silica	49·79	47·05	48·76	49·39	25·65
Alumina	6·67	5·16	4·99	6·00	2·83
Lime	22·54	23·77	23·26	22·46	6·30
Magnesia	12·12	15·35	15·78	13·93	5·39
Oxide of iron	8·02	7·57	7·21	7·39	1·68
	99·14	98·90	100·00	99·25	

Augite from the Rhœngebirge. Specific gravity = 3·347 at 17° R.

	1.	2.	Oxygen in 2.
Silica	50·11	50·73	26·35
Alumina	6·68	6·47	3·02
Lime	18·66	18 90	5·30
Magnesia	15·72	16·91	6·54
Oxide of iron	7·55	7·26	1·64

Augite from Ætna. Specific gravity = 3·359 at 17° R.

		Oxygen.
Silica	50·55	26·26
Alumina	4·85	2·26
Lime	22·29	6·26
Magnesia	13·01	5·03
Oxide of iron	7·96	1·81
	98·66	

Augite from Vesuvius.

		Oxygen.
Silica	50·90	26·44
Alumina........	5·37	2·50
Lime	22·96	6·44
Magnesia	14·43	5·58
Oxide of iron ..	6·25	1·42
	——9·99	

Uralite from the neighbourhood of Lake Baltym, in the Ural.

The Uralite is a kind of hornblend, but possessing the crystalline form of augite*. It forms crystals from 1 to 2 lines in length, which lie scattered in a grayish green matrix. They are of a blackish green colour delicately striped on the surfaces of cleavage, of a pearly lustre, faintly transparent at the edges, and possessing the hardness of apatite. A small quantity of the crystalline matter having been carefully separated from the matrix, had, according to G. Rose, the specific gravity of = 3·150. This same quantity submitted to analysis gave

		Oxygen.
Silica	33·05	27·55
Alumina	4·56	2·12
Lime..	12·47	3·50
Magnesia, with a trace of manganese	12·90	4·99
Oxide of iron	16·37	3·72
	——99·35	

Amphibole from Kienrudgrube, near Konigsberg in Norway.

		Oxygen.
Silica	49·07	25·49
Alumina	9·24	4·31
Lime	10·33	2·90
Magnesia, containing manganese	20·29	7·85
Oxide of iron....	9·77	2·22
	——98·70	

Amphibole from the village of La Prese between Bormio and Tirano.

Silica	45·31	23·53
Alumina	11·88	5·54
Lime	10·49	2·94
Magnesia, containing manganese	14·28	5·52
Oxide of iron ..	15·93	3·26
Titanic, acid with some silica ..	0·66	
	——98·55	

* G. Rose has given a description of this mineral in Poggendorff's *Annalen der Physik und Chemie*, Band. xxxiii. p. 21.

No attempt was made to determine the quantities of fluorine which these specimens of amphibole probably contained.

Plagionite.

Lead	40·98
Antimony	37·53
Sulphur	21·49
	100·00

This result agrees very closely with that obtained by Professor H. Rose. Its formula is $4\,Pb\,S + 3\,\underline{Sb}\,S^3$.

CORRECTION OF AN ERROR IN MR. WETHERELL'S PAPER, AND NOTICE OF *Venus Morrisii*, A NEW FOSSIL SHELL. BY J. DE C. SOWERBY, ESQ., F.L.S.

The following is a correction of an error in the list of fossil shells given in Mr. Wetherell's observations on some fossils of the London clay, published in the London and Edinb. Phil. Mag., vol. ix. page 464. (No. 56, for Dec. 1836.)

Among the Conchifera *Venus incrassata* is mentioned as occurring in the Hampstead Well and at Brackenhurst; this is an error: the shell found at these places and at Herne Bay, and several other places in Kent, proves, upon careful comparison with the *V. incrassata* of Mineral Conchology, to be quite distinct; it has a much more slender hinge, and is wider in proportion to its length. Having been indebted to Mr. Morris for this discovery, which is important because the true *V. incrassata* belongs to the upper marine formation, it is desirable to commemorate the fact by naming the new shell *Venus Morrisii*. I have taken upon me to correct this error because it originated with me. J. DE C. SOWERBY.

Camden Town, Dec. 19, 1836.

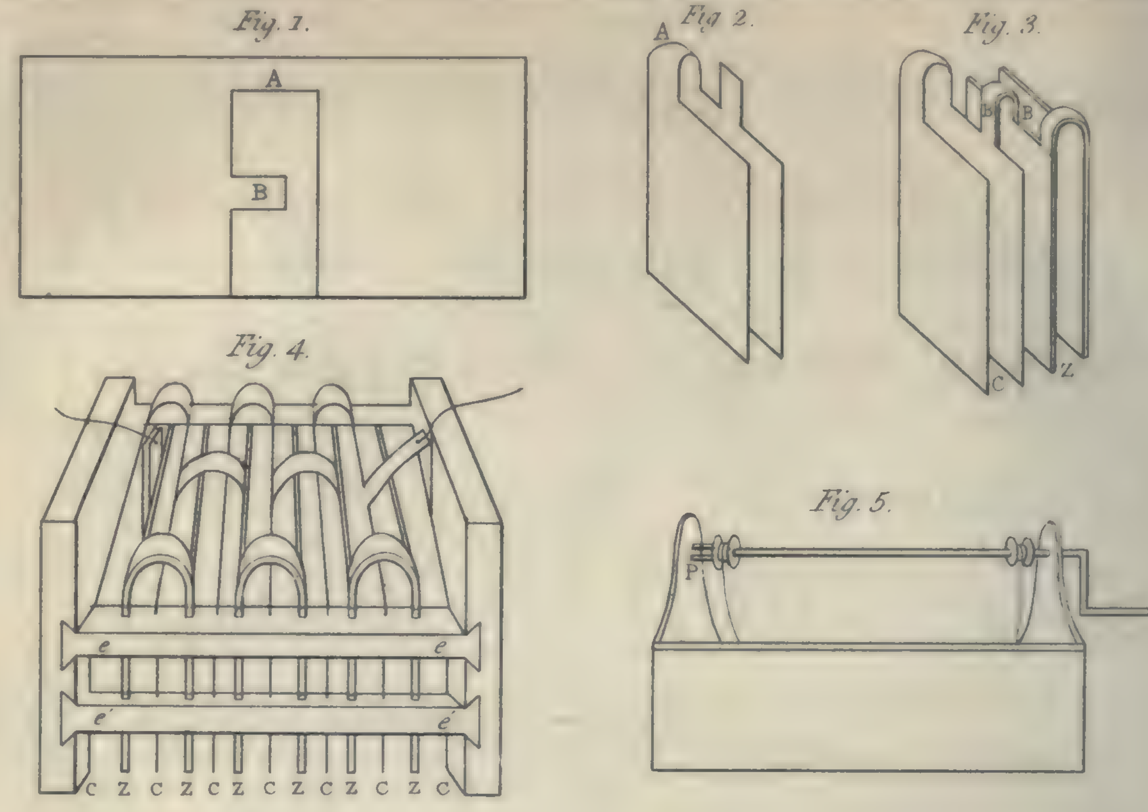
METEOROLOGICAL OBSERVATIONS FOR JANUARY 1837.

Chiswick.—Jan. 1. Clear: snowing: severe frost at night. 2. Frosty and foggy. 3. Slight thaw: hazy: clear. 4, 5. Overcast. 6. Rain: fine. 7. Clear. 8. Frosty: clear. 9. Overcast. 10. Boisterous: cloudy. 11. Clear and frosty. 12. Overcast: stormy. 13. Rain. 14, 15. Clear and fine. 16. Hazy. 17—19. Foggy. 20. Rain: densely foggy. 21. Cloudy. 22. Rain. 23. Rain: fine. 24. Fine. 25. Cloudy: rain. 26. Heavy rain: cloudy and stormy at night. 27, 28. Bleak and cold. 29. Snow. 30. Overcast: drizzly. 31. Fine.

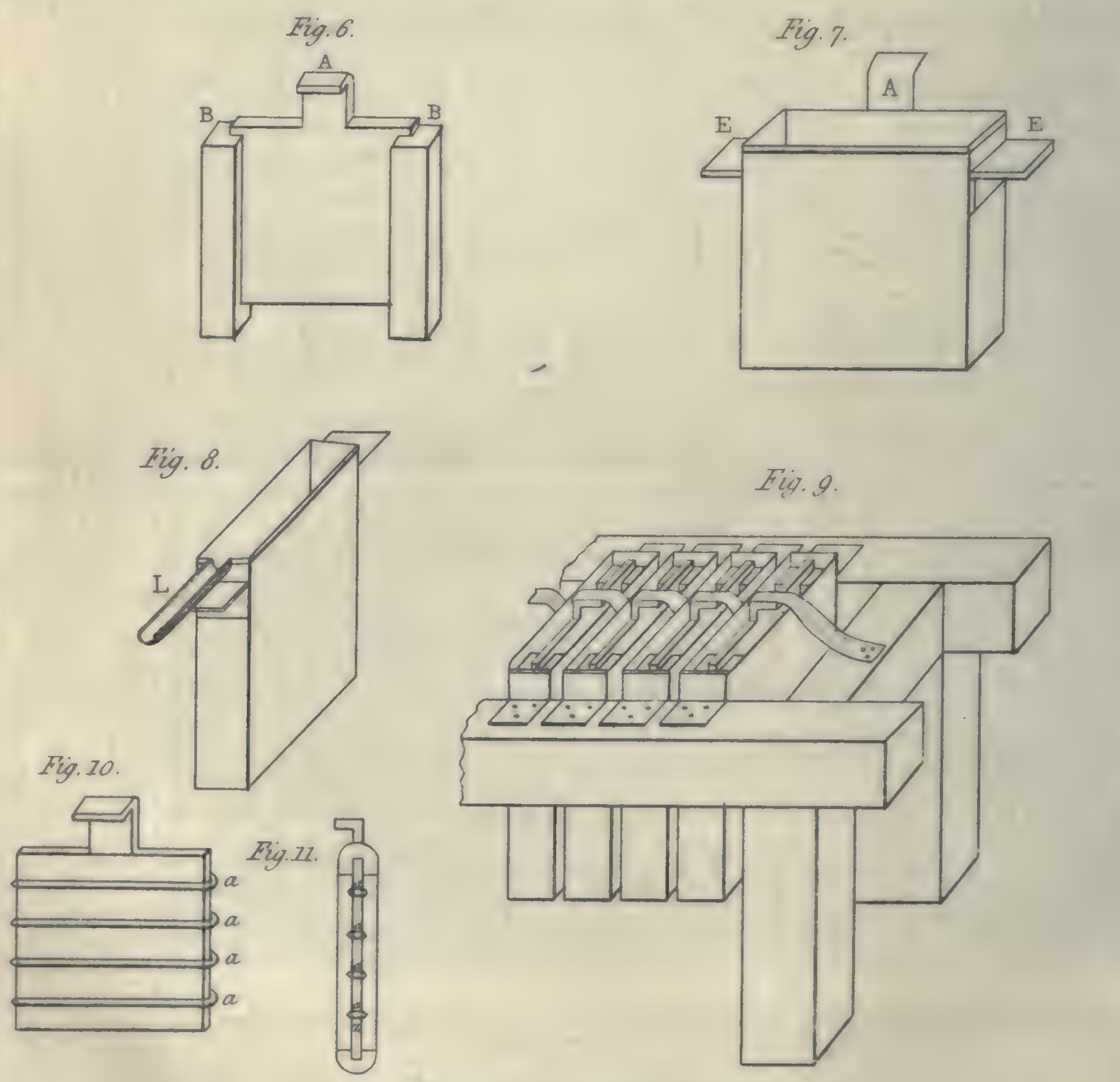
Boston.—Jan. 1. Cloudy. 2—4. Fine. 5. Cloudy. 6. Fine: rain early A.M. 7. Rain: rain early A.M. 8. Fine. 9. Cloudy. 10. Cloudy: rain A.M. 11. Fine. 12. Fine: stormy with snow and rain P.M. 13. Rain. 14. Stormy. 15. Fine. 16. Cloudy: rain A.M. 17, 18. Cloudy. 19. Cloudy: rain P.M. 20, 21. Cloudy. 22. Rain. 23. Cloudy: rain A.M. 24. Cloudy. 25—27. Rain. 28. Cloudy. 29. Snow. 30. Rain. 31. Cloudy.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary; by Mr. THOMPSON at the Gardens of the Horticultural Society at Chiswick, near London; and by Mr. VEALL at Boston.

Days of Month. 1837. Jan.	Barometer. London: Roy. Soc. 9 A.M.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ A.M.	Thermometer. London: Roy. Soc. Fahr. 9 A.M.	Thermometer. London: Roy. Soc. Self-registering Min.	Thermometer. London: Roy. Soc. Self-registering Max.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8½ A.M.	Wind. London: Roy. Soc. 9 A.M.	Wind. Chisw. 1 P.M.	Wind. Bost.	Rain. London: Roy. Soc. 9 A.M.	Rain. Chisw.	Rain. Boston.	Dew-point. Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
☉ 1.	30·510	30·585	30·506	30·22	30·5	30·7	40·8	34	12	27·5	NW.	N.	calm	...	...	...	30
M. 2.	30·392	30·500	30·444	30·10	23·7	24·0	34·2	36	22	20	SW.	W.	calm	...	...	...	24
T. 3.	30·316	30·373	30·323	30·	34·8	24·0	36·2	39	28	23	SW.	NW.	calm	·080	·30	...	27
W. 4.	30·350	30·399	30·325	30·	35·4	30·5	40·7	39	33	20	WSW.	SW.	calm	...	...	...	29
Th. 5.	30·152	30·204	29·919	29·77	36·3	34·3	38·3	44	33	34	SSE.	S.	calm	...	·03	...	30
● F. 6.	29·648	29·685	29·621	29·24	44·3	35·2	45·0	49	37	37·5	SW. var.	SW.	SW.	...	·11	·13	33
S. 7.	29·634	29·942	29·658	29·16	40·2	38·5	47·6	45	28	37	SW.	W.	W.	·130	...	·09	37
☉ 8.	30·214	30·370	30·242	29·80	34·2	34·2	44·5	46	32	33	SW.	SW.	calm	...	...	...	33
M. 9.	30·218	30·247	30·153	29·62	44·6	34·8	45·2	49	44	43	S.	SW.	SW.	...	·05	...	34
T. 10.	29·780	30·004	29·823	29·22	46·5	44·5	49·4	50	25	47	SSW. var.	SW.	W.	...	...	...	39
W. 11.	30·128	30·206	30·166	29·82	30·4	30·2	50·3	35	26	28	NW.	N.	calm	·125	...	·04	28
Th. 12.	30·118	30·147	29·655	29·77	33·7	30·5	34·4	48	33	28	SSE.	S.	calm	...	·14	...	30
☽ F. 13.	29·445	29·659	29·378	29·02	47·8	34·2	48·5	50	35	44	SSW.	SW.	calm	·044	·12	·45	38
S. 14.	29·960	30·383	29·983	29·60	37·2	35·8	50·2	40	31	35	NW. var.	N.	N.	·172	...	·05	33
☉ 15.	30·406	30·508	30·404	30·10	34·4	33·3	39·8	39	30	33·5	N.	N.	calm	...	...	...	34
M. 16.	30·346	30·479	30·333	29·96	34·4	33·3	38·4	39	33	35	N.	SW.	calm	...	·06	...	32
T. 17.	30·294	30·323	30·247	29·94	39·6	34·5	40·4	41	37	39	NNE.	S.	calm	·075	...	·18	34
W. 18.	30·164	30·191	30·017	29·78	39·3	39·5	43·2	39	33	38·5	NNE.	W.	calm	...	·01	...	34
T. 19.	29·940	29·974	29·878	29·60	35·8	34·7	40·0	38	33	37	NE.	E.	calm	...	·06	...	34
F. 20.	29·740	29·767	29·732	29·40	35·0	34·8	39·0	36	32	37	N.	W.	calm	...	...	·05	33
○ S. 21.	29·664	29·687	29·566	29·30	36·3	35·0	36·7	47	38	35	SE.	S.	calm	·063	·10	...	33
☉ 22.	29·406	29·358	29·344	29·07	44·6	36·3	45·3	52	46	42	SSE.	SW.	SW.	...	·14	...	37
M. 23.	29·424	29·444	29·382	29·00	48·3	44·6	50·5	51	46	46	SSE.	SW.	calm	·127	·05	·11	40
T. 24.	29·564	29·626	29·556	29·40	47·3	46·8	50·7	50	42	45	WSW.	S.	calm	·088	·51	·20	42
W. 25.	29·580	29·631	29·568	29·25	44·5	44·4	49·6	46	41	42	S.	S.	E.	·366	·90	·50	41
T. 26.	29·598	29·735	29·508	29·37	42·5	42·8	45·3	43	37	39·5	E.	E.	calm	·538	·18	·85	41
F. 27.	29·764	29·853	29·796	29·53	38·8	37·6	43·0	39	34	38	NNE.	E.	E.	·311	·01	·10	36
S. 28.	29·870	29·905	29·873	29·61	35·3	35·0	39·8	36	32	34	NE. var.	NE.	calm	...	·02	·05	34
☾ ☉ 29.	29·787	29·833	29·780	29·60	34·2	32·8	37·2	38	29	31	NNE.	NE.	calm	...	·20	...	32
M. 30.	29·794	29·831	29·793	29·55	36·7	32·4	37·0	44	37	35	E.	SE.	calm	·150	·01	...	32
T. 31.	29·918	29·974	29·877	29·62	42·9	36·5	43·6	49	36	41	SSE.	S.	calm	·044	·03	·46	35
	29·939	30·585	29·344	29·59	38·4	35·3	42·7	52	12	35·6				Sum 2·313	3·03	3·26	33·8



Mr. I. Young's Voltaic Battery.



Mr. De la Rue's Voltaic Battery.

J. Basire, sc.

THE

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PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[THIRD SERIES.]

APRIL 1837.

XLVIII. *An Account of a new Voltaic Battery, being a Modification of the Construction recommended by* Mr. Faraday. *By Mr.* James Young, *Chemical Assistant in the Andersonian University.**

[With Figures: Plate II.]

A PLAIN working battery, containing a considerable number of pairs of plates, arranged on the principle of the pile of Volta or the trough of Cruickshanks, is the instrument which we habitually have recourse to for illustrating chemical decompositions, and the other effects of voltaic electricity requiring considerable tension. Various constructions of this battery are in use, of which we are concerned only with that originally suggested by Dr. Hare, but of the value of which electricians were not aware till it was clearly demonstrated by Mr. Faraday†. The existence of a defect, however, is fully admitted by Mr. Faraday, in the construction which he recommends. To prevent metallic contact between contiguous copper plates, cartridge paper is interposed between them. The paper becomes saturated with the acid solution used as the exciting liquor, and the acid cannot be washed out of the paper, but is retained by it after the battery is laid aside, and may then occasion the solution of the copper, seeing that zinc no longer enters into a circle with the acid and copper, and

* Communicated by the Author.

† Dr. Hare's paper describing his battery will be found in Phil. Mag., First Series, vol. vii. p. 284; and Mr. Faraday's in Lond. and Edinb. Phil. Mag., vol. viii. p. 114.—Edit.

therefore the latter is unprotected. These papers require likewise to be renewed occasionally, and they give to the construction the character of a temporary arrangement. It is true that glass or porcelain plates may be placed between the contiguous coppers; but these are inconvenient, and, in fact, bring us back to the old construction of the trough partitioned into cells. It is to be noticed too that these copper surfaces, with the paper between them, are lost, and turned to no account in collecting electricity.

After constructing several batteries with the interposed papers, and becoming fully sensible of the annoyance which the papers occasion, an arrangement of the plates suggested itself which does not require interposed papers, and in which both surfaces of the copper as well as of the zinc plates are made available. Within the last eighteen months I have constructed several dozens of instruments of the construction to be described, and having compared them experimentally with batteries of the same extent of copper and zinc on Dr. Hare's construction, I am prepared to state that, from the same surfaces of zinc, electricity the same in quantity and tension is produced in both forms, but that in the new construction this effect is produced with half the quantity of sheet copper, which arises from both sides of the copper plates being presented to surfaces of zinc. The new construction has, I believe, all the advantages of approximation of the plates and compactness of Dr. Hare's battery, which have been pointed by Mr. Faraday, without the great and acknowledged drawback of the interposed papers.

The sheet copper and sheet zinc to be used in this battery are first cut into long ribbons, of the breadth which it is intended to give the plates. Suppose the ribbons two inches broad; both the copper and zinc ribbons are then divided into lengths of five inches, and a portion cut out as in fig. 1. The slip is thus divided into two squares of two inches each, which are connected at A, and a piece is left projecting at B. The zinc and copper sheets are cut up exactly in the same way. Fig. 1 therefore represents either a single zinc or a single copper plate. The plate is then bent at A, and presents the appearance represented in fig. 2. In fig. 3, we have two plates, one of copper C, and the other of zinc Z, which are exactly alike in construction, but are placed differently, as shown in the figure. Thin projecting parts B B are soldered together, and this is the only metallic communication between them which is allowed to exist. Fig. 3, therefore, is only one copper and one zinc plate, or it is one pair of plates. Each pair is made up in the same way. In arranging a num-

ber of pairs to form a battery, they are interlaced so that a copper square comes in between each couple of zinc squares, and a zinc square between each couple of copper squares. It is easy to see how this arrangement can be made, when the plates are in the hand, though it is difficult to describe it. At the positive end of the battery there is a single copper plate, which is soldered at the top to the last double copper plate, as seen in fig. 4; which figure represents three pairs properly arranged, and also the manner in which they should be fitted up and kept steadily apart in a wooden frame. This frame consists of two cross-bars, *e e*, *e' e'*, in front, and the same behind, dovetailed into solid ends. The channels in the cross-bars, for the reception of the edges of the plates, are formed by placing the four cross-bars together, and sawing a little way into one side of them all, every eighth of an inch or so in their length, so as to form a set of parallel grooves. We have by means of this frame a much greater security that no metallic contact will occur between contiguous plates, than when they are separated by wedges of cork, as in Dr. Hare's construction, which may slip out.

The frame and plates are introduced into a trough, which may be of wood or stoneware, containing the exciting liquor. Dr. Hare's revolving arrangement of the two connected troughs may be adopted for this battery, although we have been led to give a preference in practice to a single trough to contain the frame. To the solid ends of the frame are attached two cords, which are fixed to two pulleys, on which they are wound up, on turning a winch, as represented in fig. 5, by which means the frame and battery can be raised out of the fluid. If the axis (a stout wire) on which these pulleys are fixed can be moved a little backwards and forwards on its bearings, it is easy, by means of a little projecting peg at P, which fits into a hole in the side of the pulley, to fix and support the frame in a position above the trough, and out of the exciting fluid, when that is desirable. But the form of the trough to contain the frame and plates may be varied according to the object in view, or the purposes to which the battery is to be applied.

In comparing a battery of the form described above, either with Dr. Hare's or any of the other forms in use, it is to be remembered that the plates or elements of the battery are all of double the size they appear to be, or that in this construction you have half the number of pairs, but each of double the dimensions of a pair in any of the old batteries having the same appearance.

A small battery of this construction, containing twelve pairs,

of two inches breadth, of plates (the size which we have taken above as an example), may be contained in a trough eight inches in length, and will evolve, when its terminal wires are soldered to a Faraday's volta-electrometer, six or seven cubic inches of the mixed gases in three or four minutes, with a charge of half an ounce of sulphuric acid and half an ounce of nitric acid, in twenty-four ounces of water, (all by fluid measure,) and is therefore amply sufficient to demonstrate the decomposition of water on a considerable scale.

It is proper to use the thickest sheet zinc which can be had, in the construction of the plates, although the thinnest sheet copper will suffice, from its being so well supported. When the zinc plates are worn out, the cross-bars may easily be pulled out of the solid ends, and the elements of the battery separated. New zinc plates being soldered to the old coppers, the whole may again be quickly rearranged in the old frame.

Glasgow, Jan. 4, 1837.

XLIX. *On the Effects of a Voltaic Battery charged with Solution of Sulphate of Copper.* By Mr. Warren De la Rue.

[With Figures: Plate II.]

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In answer to your query (vol. ix. p. 484,) as to the relative effects of batteries charged with sulphate of copper or with acids, I beg to submit to your attention the following facts and deductions.

It is well known that in connecting the poles of a battery with a definite length of wire, the wire will become ignited, and continue so for an exceedingly short space of time after immersion in an acid; and if the battery be immersed without connecting the poles and allowed to remain for a few minutes, and the connection be then made,—with the same length of the same wire,—no ignition whatever is produced*. As in this latter case no zinc can have been deposited on the copper plate, prior to the connection of the poles, it follows that this decrease of power must result from some other cause. At the moment of immersion in dilute acid, say sulphuric, the electricity is produced by the combination of the acid with that portion of oxide which is in perfect contact with the zinc plate: when this thin coating of oxide is removed, the zinc plate is then oxidized at the expense of water, hydrogen be-

* A battery regains its former power by exposing the plates to the action of the atmosphere.

ing set at liberty; and as the hydrogen assumes the gaseous form, it annuls or carries off a large portion of the electricity*.

If sulphate of copper be used in charging the battery instead of acid, oxygen is supplied to the zinc by the oxide of copper; no evolution of gas, therefore, takes place†; and the action is thus rendered continuous, the effect being fully equal to that momentarily produced by immersion in acids. The fusion of metallic points of *very large dimensions*, the decomposition of fixed alkalis, &c. &c., cited in my former communication as the effects of a voltaic battery of 100 pairs on Cruickshanks's construction, cannot be produced by the same battery when charged with acid, the momentary power being exhausted before the battery can possibly be brought into action ‡.

The following is an experiment to ascertain the effect on the battery produced by the deposition of copper on the zinc plate. Fig. 10 represents one of the zinc plates of the battery: round it at (*a*) are placed four copper wires one tenth of an inch in diameter; these are each attached to the plates by a drop of solder. Fig. 11 shows the zinc plate surrounded by the copper as in Wollaston's plan: the battery consists of twelve such series. It is clear that there are four small local currents in each cell; yet the power of the main current is increased, and not diminished as I conceived it would have been. From the proximity of the copper wires to the zinc plate, there is no deposition of copper on the zinc plate; it adheres to the copper wires so firmly that it is exceedingly difficult to remove.

I find that amalgamating the zinc greatly increases the power of the battery, and prevents the strong adherence of the copper to the zinc plates, which are therefore cleaned with facility§. I have a battery of 30 pairs of four-inch plates

* A similar effect takes place in the formation of steam, which causes the gold leaves of an electrometer to diverge with *negative* electricity.

Professor Faraday has shown that a voltaic current ceases to affect a magnetic needle if employed in the decomposition of a solution of iodide of potassium: hydrogen is given off in this case. Again, if a battery be employed in decomposing a solution of a metallic salt, an atom of it will be decomposed for the solution of every atom of zinc; but this does not destroy the current, for an atom of water is at the same time resolved into its elements.

† The exceedingly small quantity produced by the local action cannot be taken into account.

‡ This fact I am ready to prove by actual experiment to any scientific gentleman who will do me the favour to call for that purpose.

§ The zinc plates are amalgamated by rubbing them with dilute nitric acid and mercury; the mercury is allowed to be absorbed by the zinc plate, and the operation must then be repeated, which is a requisite condition.

with amalgamated zinc, which is well adapted to the use of sulphate of copper; it is more œconomical in its construction* than any now in use, and possesses this great advantage, that the zinc when worn out may be easily replaced. Fig. 6 shows the zinc plate, which must be tinned on the top A† prior to the amalgamation of the rest of the plate; B B are two slips of wood grooved out to within three fourths of an inch of the bottom, and intended to retain the zinc plate in its proper position. The copper plates are formed into cells, as represented in fig. 7, five inches square and one inch wide: E E are two ears of copper, by which the cell is suspended in its place; A is a slip of copper to form a connection by means of solder to the zinc plate in the adjoining cell. The cells are painted on the outside to protect them from the action of acid. The zinc plates do not descend lower than within three fourths of an inch of the bottom of the cells, so that the space left may contain the deposit resulting from the decomposition of the sulphate of copper. The cells are supported in a long wooden frame by the ears E E and retained in their place by tacks driven through them as represented in fig. 9. Fig. 8 represents a contrivance by which the charge may be renewed while the battery is in action: at the top of each cell may be placed a lip or spout L, a quarter of an inch deep; these must overhang a wooden gutter running the length of the frame. The solution must be renewed with a funnel having a long neck, the long end being inserted nearly to the bottom of the cell; when fresh solution is poured in, the spent liquor will run out of the lip into the gutter.

Immediately after a series of experiments the battery must be emptied, and the plates well cleaned by dashing water between the cells. If this be not immediately attended to it will be exceedingly difficult to remove the deposit from the cells. A Cruickshanks's battery is best cleaned by laying it on its side. I remain, Gentlemen, yours, &c.

110, Bunhill Row, Dec. 7, 1836. WARREN DE LA RUE.

P.S. The fact related in your first note (vol. ix. p. 484) shows that Professor Daniell had no intention of employing sulphate of copper in an ordinary battery; he immersed a zinc plate in sulphate of copper and found that there was local action, from the deposition of copper; but he went no further.

* The one I have, cost altogether 3*l.*; but as I put this together myself, a similar battery constructed by workmen would of course be rather more expensive.

† This is effected by filing the top smooth, wiping it over with a little muriate of ammonia, and then dipping the top in a ladle of melted tin, a little tallow being placed on the tin to prevent the surface from oxidating.

L. *A Report of the Progress of Phytochemistry in the year 1835, in reference to the Physiology of Plants.* By J. Cl. Marquart.*

THE experiments on starch which were mentioned by M. Meyen in his Annual Report on Physiological Botany for 1834 †, have been continued in various ways. According to the experiments of MM. Payen and Persoz ‡, starch consists of 995 p. m. *interior substance*, which they call amidone, and 3 p. m. *teguments*. The other two thousandth parts they reckon as carbonate and phosphate of lime with silica. They found also in the starch of native plants a disagreeably smelling oil, soluble in alcohol, which was not contained in exotic starch. Amidone is insoluble in water as long as it remains unchanged; it only swells in it, and then passes through several filters.

The apparent solubility of amidone in water of 65—70° [Cent.] is therefore properly only a suspension of it, effected by means of the greatest divisibility possible. The known qualities of starch solutions belong to this amidone; neither diastase nor iodine, &c., act on the tegument, whose nature is not further characterized. The authors also critically illustrate the results of M. Guerin's experiments (Annual Report for last year, p. 145), and consider his amidine to be their amidone in a state of great division, with a portion decomposed by his method of experimenting; on the contrary, they regard his *amidin soluble* to be *amidone* swelled by water. M. Guerin soon answers these objections §, and maintains, supported by experiments, that the amidone of MM. Payen and Persoz consists of a soluble and an insoluble part. The *dextrine*, which MM. Payen and Persoz formerly considered to be the contents of the starch granules, is a product of the action of acids on the amidone and is a mixture of sugar, gum, and amidone.

M. Guerin Varry studied || more attentively the action of diastase on the starch granules, and found the doubt, already expressed by M. Meyen in the Report for last year (p. 151) to be true, that diastase is capable of rending the tegument of the starch granule. The starch, which had been left unchanged by hot water, remained, according to M. Guerin, quite unchanged by diastase even by +20—26° Cent.; the amidone, on the contrary, is changed by it partly into sugar

* From Wiegmann's Archiv für Naturgeschichte, vol. ii. part iv. p. 131.
† See Wiegmann's *Archiv*, vol. i. p. 143.
‡ *Annales de Chimie et de Physique*, Août 1834, p. 337—371.
§ *Ibid.*, Sept. 1834, p. 108, 109. || *Ibid.* Sept. 1835, p. 32—78.

even at 0°; it is even capable at −5−12° of rendering the paste of starch liquid without at the same time forming sugar. The conversion of starch into sugar takes place very rapidly, as well in the air as *in vacuo*, and without the absorption or disengagement of gas.

M. Guerin examined the starch under the microscope, and appears, as is often the case with the French, to have had no knowledge of the experiments made by the Germans on the same subject. He observed, like M. Fritsche, the concentric rings and the nucleus of the granules, and very improperly names the latter the umbilical point; he also found the *monstrous granules* of M. Fritsche, and describes them as a connection of one or two granules which, on account of want of space in the *mother cell*, had forced themselves on one another so that the formation of hexagonal cells by the aggregation of the original round cells could plainly be observed. The umbilical point of each granule was always directed outwards. M. Guerin, after having in this manner studied the form of the granules, made experiments on the action of water on them at various temperatures, sometimes using pure water, sometimes water and diastase; from which it appears in general, that at 50—53° C. the water with or without diastase had no effect on the granules; at 54—55° he perceived some torn granules, and at 59—60° several granules quite torn, and many already empty; at 62° all the granules torn and empty, yet so, that in diastase the husks were only split, but in water broken down into shreds, (*gefranzt*).

Of more importance are the remarks of M. Hartig on the appearance and function of starch in the vegetable kingdom, in his memoir "On Starch, *Cambium*, the nutritive sap and milk-sap of woody plants, in reference to phytochemistry, chemistry, and common use*." M. Hartig found in the woody body of all deciduous trees after the fall of the leaves a great quantity of starch (up to 26 per cent.), which in spring, as soon as the sap begins to ascend, gradually diminishes from the periphery to the centre, being dissolved by the carbonated water of the ascending stem. With this solution there appears to be connected an action on starch similar to the known action of the weak mineral acids in the first moments; namely, the reaction of iodine has ceased and gum is formed, the solution of which forces its way through the medullary rays to the bark and here forms the basis of the young wood. For according to the author, *Cambium* is the young cellular tissue, overcharged with sap, forming active juices (Bildungssäfte), and not

* Schweigger and Erdmann, *Journ. für praktische Chemie,* vol. v. part iv.

the descending sap which has been prepared in the leaves, since we find it already in the stem when the leaves have not yet sprouted, and only completely developed leaves are capable of an assimilating process.

M. Du Menil found in his examination of the bark of *Pinus sylvestris*, in 1000 parts, 60 parts starch*. According to Du Menil the bark was separated from the alburnum; as however non-botanists possess in general erroneous conceptions of bark, liber and alburnum, we do not know what Du Menil has examined; very likely what he separated from the bark was the young wood. M. Nardo, who examined the bark of *Pinus maritima* †, found in it no starch, and the question is, whether this difference of results depends on the time of the year, or on the method of examination. M. Proctor ascertained the occurrence of starch in the bark of *Prunus virginiana* ‡, as did J. Martin in the leaves of *Cassia marylandica* §. M. Payen || examined the tubers of *Oxalis crenata*, which were recommended as food, and found in the younger kinds 2·5 per cent. starch, and in the older ones 10 per cent. Those granules of starch, which are inclosed several in one cell, are more uneven and irregular than those of most of the other kinds. The results of an analysis of the tubers of *Cyperus esculentus*, by M. Semmola ¶ deserve also to be mentioned; among other substances, he states that he found 224 p. m. starch, and 43 p. m. inulin; these results, however, want confirmation.

M. Julia Fontenelle** made known some interesting remarks on corn which had been buried in the earth for a considerable time. A corn magazine in the citadel of Metz, built about 300 years ago, contained corn from which good bread could be made. M. Passalaqua brought from the ruins of Thebes, some species of fruit, the production of which he supposes to have taken place above 3000 years since. By examination the corn was found to be a little acid, to have lost its gluten, but to have *retained the whole quantity of starch*. A bread, even as old as the above-mentioned corn, found in a mummy, contained a quantity of germinated and lightly roasted grains of barley, which also contained an acid, no gluten, and much starch.

Inulin was formerly obtained only by boiling the parts of plants, by which operation it sank as powder from the hot decoction. After what we have learnt on the changing action of boiling water on starch, and those bodies related to it,

* *Archiv für Pharmacie*, vol. i. Part i.
† Isis, 1834, Part vi. vii. p. 670.
‡ *Journ. de Chimie Méd.*, 1834, p. 674.
§ The American Journ. of Pharm., 1835, April, p. 19—24.
|| *Journ. de Chimie Méd.*, 1835, May. ¶ *Ibid.* ** *Ibid.*, 1834. Feb.

inulin could not be considered as an unchanged part of the plant, and I (M. Marquart) therefore made several experiments on it* to separate the unchangeable matter, to which inulin owes its origin. I succeeded in insulating from thick roots (tubers so called) of *Georgina variabilis* a milky liquid (by the method for making starch): the milkiness, as could be perceived on its being greatly magnified, was caused by very small globules, which were quite diaphanous and round, differing from the starch granules in not settling from the liquid and not being coloured blue by iodine. I, however, succeeded in separating them by freeezing the liquid, and was able to wash them with water and to examine them more closely. I discovered in them the basis of the substances which have been named Inulin and Dahlin, and intend to give to the first the name of *Synantherine*, on account of its occurrence in the *Compositæ* or *Synantheræ*, and to consider it as a substance analogous to starch. I propose for the two last substances the name of Sinistrine, on account of their analogy with Dextrine, as Biot and Persoz have already discovered that inulin turns the plane of polarization to the left.

M. Kœne, who examined the roots of *Anacyclus Pyrethrum*, extracted from them 57 per cent. of inulin (sinistrine)†, which is remarkable on account of the quantity, if there is no error in his statement. The mealy sediment of lichens (*Flechtensatzmehl*), the original form of which we are yet ignorant of, presents a similar case. M. Guerin‡ has also examined this, but only the soluble part, which he calls *Lichenin*. He prepared it from *Cetraria islandica*, by filtering the hot decoction and precipitating it with alcohol. After dissolving and precipitating several times, it is in the dry state yellowish, swells in water, and is then void of colour, smell, and taste. Lichenine dissolves easily in hot water, less in cold; the solution *is coloured blue by iodine*, but weaker than by amidine, with which it has the same elementary composition, namely $C_5\ H_{11}\ O_5$. It forms sugar with sulphuric acid, and with nitric acid oxalic acid in a much larger quantity than any other substance of the vegetable kingdom, so far as we at present are acquainted with vegetable substances.

M. F. Nees von Esenbeck§ wrote a supplement to my [M. Marquart's] memoir on Inulin, in which he expresses the opinion, that the so-named Bassorin is the insoluble matter of the Bassora and tragacanth gums, the membranes of the torn

* Geiger and Liebig's *Annalen der Pharmacie*, x. 1.

† *Ann. de Chimie et de Physique*, July 1835.

‡ *Journ. de Chim. Méd.*, ix.

§ Geiger and Liebig's *Ann. der Pharm.*

cellular tissue, and endeavours to prove it from the nature of this insoluble matter and from the manner of excretion of these kinds of gums from the bark. According to the experiments of Guerin * the elementary composition of this bassorin is different from that of arabin (of the white soluble gum), while cerasin, the gum of our *Rosaceæ*, is only distinct from arabin by its difficult solubility, and by its being, when long boiled with water, completely converted into arabin. Bassorin consists of 10 m. g. carbon, 11 m. g. hydrogen, and 11 m. g. oxygen, while arabin is composed of 12 m. g. carbon, 10 m. g. hydrogen, and 10 m. g. oxygen. According to M. Guerin, starch has exactly the same composition as arabin; he considers, however, starch to consist of amidine and *amidin tégumentaire*, and does not therefore call both isomeric. *Amidin tégumentaire* has, according to Guerin, the same elementary composition as the woody fibre, namely, $C_7 H_{10} O_4$, which is not without influence on the views of the structure of the granules of starch, and serves as a support to those who consider the starch granule as consisting of a soluble interior and of a husk.

We have seen that by treating starch with acids and diastase it was first converted into gum, and then into sugar; have learned that gum and starch have the same composition; and are now informed by Liebig's experiments, that crystallized cane sugar is isomeric with pure gum, or is equivalent to it in its elementary composition.

In physiology, the terms gum and mucus have the same meaning, although chemistry distinguishes them. Those muci which are stated by chemists to contain nitrogen merit a more close examination. Thus M. Tromsdorff † lately found 7·5 per cent. of a mucus, soluble in water, containing nitrogen, in the fruit of *Coriandrum sativum*; M. Payen ‡, 0·1 per cent. of a like substance in the tubers and stem of *Oxalis crenata*. M. Herberger § examined *Sphærococcus crispus*, and found in it 79 per cent. of a substance which he calls the gelatine of algæ, which according to our opinion, founded on the properties of the matter mentioned, does not differ from common gum, being soluble in water and precipitated from the solution by alcohol. The small proportion of nitrogen is not essential, or is caused by some mixture. We consider the algæ gelatine of M. Herberger as cellular membrane, it being insoluble in water and differing only from bassorin by the small quantity of nitrogen it contains.

* *Ann. de Chim.*, vol. xlix. p. 248.

† *Archiv für die Pharm.*, iii. 2.

‡ *Journ. de Chim. Méd.*, Mai, 1835.

§ Buchn. *Repert.*, vol. xlix. part 1, 2, 3.

M. Lassaigne* found uncrystallized sugar not only in the plants already known to contain it, but also 1 per cent. in the leaves of *Morus alba*. M. Zenneck found nearly 6 per cent.† in the fruit of *Panicum miliaceum*, and M. Semmola‡ found 12·5 per cent. of crystalline sugar in the radical tubers of *Cyperus esculentus*. M. Malagutti§ has given a very good paper on sugar, from which we may remark that cane sugar is hydrated, *i. e.* changed into grape sugar, by all weak acids without exception. The latter has been found in all acid fruits; from this the process necessary to its origin becomes evident. If the action of the acids on the grape sugar continues, water is again taken from it, and ulmic acid is formed. If at the same time the atmospheric air acts on it, oxidation takes place, and by this the formation of formic acid.

The crystalline part of manna, *mannite*, was found by M. Winckler in a preparation from the buds of the poplar ||, and according to Boutron-Charlard and Guillemette¶, the grenadin found in the bark of the roots of *Punica Granatum* is also mannite. Grenadin acquires from this circumstance more importance, and deserves a closer examination as to its occurrence.

We here again direct attention to the rather too much neglected experiments of MM. Fourcroy and Vauquelin on the sap of *Allium Cepa*, and of Laugier** on the sap of *Daucus Carota*. All three chemists found in the saps of those plants, after they had begun to ferment, crystalline manna, which they were not able to extract from the fresh saps. At the same time the fermented saps indicate free acids, and, according to Laugier, the manna from *Fraxinus* has in its fresh state a smell of acid. Perhaps the natural or artificial process of originating mannite might be a task worthy of chemical research.

M. Buchner, jun.†† examined the sap of the nectaries of *Agave geminiflora*, which flowered in autumn, 1834, in the Botanical Garden of Munich. It possessed the consistence of a thin syrup, specific gravity = 1·09, and contained a great quantity of uncrystalline sugar, water, and traces of gypsum. The sweetish putrid smell disappeared when in contact with the air. M. Buchner, sen., examined some time back the sap of the nectaries of *Agave americana*, and Anthon that of *A. lurida*, which exhibited nearly the same qualities.

[To be continued.]

* *Journ. de Chim. Méd.*, 1834.

† Buchn. *Repert.*, vol. xlix.

‡ *Journ. de Chim. Méd.*, 1834, p. 676.

§ *Journ. de Pharm.* Sept. 1835.

|| Buchn. *Repert.*, vol. li. part 1.

¶ *Journ. de Pharm.*, April 1835.

** *Mém. du Mus. d'Hist. Nat.*, vol. iv. page 102—108.

†† Buchn. *Repert.*, vol. li.

LI. *On a Double-bodied Intestinal Worm, the* Syngamus trachealis. *By Dr.* Charles Theodore von Siebold, *of Danzig.**

THIS remarkable parasitic animal, which I will here describe, was discovered about twenty-five years ago, but its internal structure was imperfectly known, and the attention was not paid to it which it merited. At the present time it appears to be quite forgotten, and I therefore present it again to the friends of helminthology with its true structure and with a new name; perhaps I shall be able to procure for it a determinate place in the system †.

The worm of which I speak must, according to its structure, be classed with natural monsters, and may justly be placed by the side of *Diplozoon paradoxum.* The *Diplozoon paradoxum* ‡ can no longer be considered as the only known double animal, for our *Syngamus trachealis* is also one, but with this difference, that it does not consist of two hermaphrodite animals closely connected together, but that a *male* and *female* animal are grown together.

Before I begin the description of this monster I will state what is already known of it. Montagu described this worm as a *Fasciola* §, and has given an indifferent drawing of it ||, and mentions as its habitat, the *trachea* of young hens, pheasants, and partridges. He found twenty such worms in one windpipe. Montagu ascribed to it a disease known in England by the name of *gapes*, which often seizes the young poultry a short

* From Dr. Ar. Fr. Aug. Wiegmann's *Archiv für Naturgeschichte*, Part II. 1836, p. 105. Translated by W. Francis.

[† Since the publication of this paper, a memoir by Nathusius, inserted in the first number of Wiegmann's *Archiv* for 1837, proves the *Syn. trachealis* of Siebold to be a species of *Strongylus* in the act of coïtus. A translation of this paper will appear in a future number of our Journal.—Edit.]

‡ I here take the opportunity of adding to the observation which has been made on the circulation of the blood in the *Diplozoon*, (see Nordman's *Beiträge*, p. 69, and my first paper in Wiegmann's *Archiv*, Part I., 1835, p. 58.), the following correction. New observations which I have made on this animal since the ciliary motion of Purkinje was made known, have convinced me that the motion of the blood is not perceived in the vascular system of this animal, but that the movement arises from the inner surface of the vascular membrane, as Ehrenberg has lately stated in Wiegmann's *Archiv*, Part II. p. 128. The vessels possess on the inner surfaces moving cilia, which exactly imitate the motion of the blood. This movement resembles the ciliary motion described by Henle (Müller's *Archiv*, ii. 6, p. 576.) in the tortuous canals of the *Branchiobdella parasita*, and which I also have seen in this animal.

§ Account of a species of *Fasciola*, which infests the trachea of poultry, with a mode of cure, by George Montagu, in the Memoirs of the Wernerian Natural History Society, vol. i. for the years 1808, 9, 10. Edinb. 8vo, p. 194.

|| Ibid. Pl. vii. fig. 4.

time after their appearance from the shell, and kills them. His account of the disease is as follows (p. 194): it "is a frequent gaping, attended with an extension of the neck, like suffocation, and sometimes an apparent phthisical affection or irritation of the lungs." The neck and lungs of the fowls in which this worm was discovered were found much inflamed.

The supposition of the editor of the Wernerian Memoirs (p. 199.), that the worms, discovered also in the windpipe of hens and turkeys by Dr. Wiesenthal of Baltimore (in the *Medical and Physical Journal*, 1799, vol. ii. p. 204.) resemble those described by Montagu, I cannot confirm, as in those the characteristic lateral process is wanting*. Rudolphi believed Montagu's worm to be the *Distomum lineare*, which he had discovered in the mucous skin of young poultry†; if, however, Rudolphi's description of *Distomum lineare*‡ be compared with that which the English naturalist has given of his *Fasciola* §, there will be found a great difference between the two animals.

This is all that I have been able to find in scientific works on this worm, and what satisfied me less, was to find upon close examination of this worm how little its true structure was known to Montagu. I have been fortunate enough to find this parasitic animal in the windpipe of three different species of birds, namely, in *Phasianus Gallus*, *Picus viridis*, and *Cypselus apus*. I discovered a single one at first in October 1833 at Heilsberg in a very lean hen; in May of the following year I found among eleven chimney swallows, which were taken in Heilsberg, in one bird two individuals at once, and a fourth in a green woodpecker which was shot this year near Danzig. I have in all my dissections of birds during two years, in which time I have examined a great number, always searched the *trachea* and its branches, and have found the *Syngamus trachealis* only in the three birds mentioned; so that I must believe the worm is here a rarity, and only occurs occasionally and alone, while in England it is found so often and collected in such numbers in the windpipes of poultry, often committing

* I was not able to compare the work of Dr. Wiesenthal more closely.

† Rudolphi, *Synopsis*, p. 414.

‡ Rudolphi, *Historia Natur. etc.* vol. ii. 1. p. 414.

§ Wern. Soc., p. 197. " Body round, acuminated at the posterior end, the lower aperture produced on a long stalk or arm, that extends rather beyond the anterior end of the body, where the other aperture is placed, and is not above half the size of that part: these openings spread a little, or are sub-infundibuliform; the larger appears to be the mouth, and is slightly sexpartite; that on the arm is used as a sucker, and is the part by which it adheres to the inside of the trachea: the divarication takes place at about one fifth part of the length of the body: the colour is red, and the intestines, which are extremely numerous and tortuous, are white."

great ravages on the feathered tribe, that their owners have been obliged to have recourse to a remedy for it.

The worm consists of an extended cylindrical body, divided towards the upper part into two long branches: the two branches vary in thickness. The thickest branch forms properly the anterior extremity of the body, and if we reckon it to the whole length of the body, the other and more slender branch extends from the junction of the middle and anterior third of the body, making an acute angle with the anterior extremity. The whole length of the body is nearly half an inch, its thickness towards the hinder part $\frac{1}{4}$ to $\frac{1}{3}$ lin., the thinner branch is $1\frac{1}{4}$ lin. in length and measures about $\frac{1}{6}$ lin. in diameter. In order to distinguish better the two branches, I shall call the thick one the female branch and the thinner the male branch, which I hope toprove in the course of this memoir. The male branch is sometimes shorter, sometimes longer than the female branch. The motions of this animal are very slow. Its colour is brick-red, and equals in brightness that of the lungs of birds: at both heads the red passes into yellow. The whole worm is transparent, and the red-brown intestine and the convoluted generative organs are visible through its integument. If the worm be kept for some time in water, the red colour disappears and a dirty yellow takes its place; the colour of the male branch is always fainter than that of the body. The heads of both branches are of the same structure, only that of the male is inferior in thickness to that of the female. The description of one branch may therefore suffice for both. I found the worm at times with the head of one of the branches, at times with the other, fixed to the mucous membrane of the windpipe. This cephalic extremity, the upper free end of both branches, is always inflated, and is contracted at the place where the head, which by this operation has taken the form of a ball, passes into the branch: exactly in the middle of the head is a round wide aperture or mouth. The female branch is very little bent; the male, on the contrary, has often the form of an S or winds round the female branch. At the place where both branches join is perceived, in the obtuse angle which the male branch makes with the body, a slit-like aperture or fissure (see Plate I. fig. 6. *c.*), out of which I have seen the eggs glide, and in which therefore we have to look for the *vulva*. The body is bent down to its most inferior part in a soft undulate form, and at its end is round and truncated; from the middle of this blunt end of the body descend sometimes shorter sometimes longer arms or stalks. An anus was nowhere to be found.

On examining the interior part of this animal, I found as follows: The exterior skin surrounds, as in the *Nematoidea*,

a cavity in which the intestines lie inclosed. The intestines are here bathed by a red liquid, in which only a few vesicles and particles swim. This is the liquid which imparts to the worm the beautiful red colour. Whether the cavity of the male branch stands in connexion with the cavity of the other parts of the body, which are continued into the female branch, whether it is separated from it by a partition, I could not determine, on account of the small number of specimens which I had to examine. On injuring the skin the red liquid flowed out without the intestines being pressed forwards ; from which I conclude, that the skin which covers the intestines does not possess that elasticity which is common to the skin of the cylindrical worms. The skin was almost even, and not as in most of the *Nematoidea*, annulate.

The circular mouth-aperture leads to a cavity of the form of a basin, which does not consist, as in the *Distomum*, of a muscular tissue, but of a firm horny substance of a dark colour, which is visible through the skin : its outer border has six indentations, and is so inclosed by the tegumentary covering that it is everywhere kept at the same distance from it.

Opposite the mouth the basin is pierced by a small aperture, through which we arrive at the œsophagus. I saw this aperture surrounded by six minute knots or hooks. As I now turn to the description of the digestive organs, I shall only speak of the male branch. The œsophagus of the male branch is very much prolonged, begins small at the inferior small aperture, increases in the middle, and ends rather rounded. Its colour is dirty yellow ; its structure, like that of many *Nematoidea*, is very muscular, and is traversed by a trihedral canal. Behind it commences the somewhat broader, more simple, and red-brown-coloured intestine, which passes in a tortuous manner to the branch, and terminates in a *cul-de-sac* before the connection of the latter with the body. The œsophagus of the female branch is just the same as the former as to colour, situation, and structure, and is only shorter and more compressed, from which it appears in the shape of a pear turned upside down. The same red-brown simple intestine which arises from it, is somewhat broad, extends in a more tortuous manner through the whole body, and finishes in a *cul-de-sac* before the caudal extremity. Both intestinal canals contain very small granules, which may be well compared with pigmentary molecules. On opening the intestinal canal only a few of these molecules force themselves out, the rest remain attached to the inner surface of the intestine. If a piece of the intestine is examined under a microscope, it has just the appearance as if it possessed vascular reticulations, which is caused by the molecules being separated into several heaps

and isolated specks, and by leaving unfilled furrows, which cross each other, and give it the appearance of a vascular net.

Next to the intestinal canal, the generative organs of this worm are the most striking. In the male is perceived a dirty-white very thin vessel, which begins by an unattached slender extremity near the middle of the *œsophagus*, and winds itself down the intestine. The vessel becomes larger and larger as it descends; and, with the last bend which it makes at the lower end of the branch, it suddenly contracts and runs down as a very slender canal to the above-mentioned slit. I could not properly follow the end of this canal, as it was very indistinct before the slit, and partly covered by two small long and thin bodies. These long bodies, which lay in the hinder extremity of the male branch, bent near the slit, consisted of a horny substance, and touched one another at an acute angle, directed downwards. A fine white granular matter formed the contents of this receptacle. The vessel could easily be taken out entire by cutting off the branch.

A much more compound vessel, with the intestinal canal, occupied the female branch and the other parts of the body. There were here two vessels with blind ends which began in the lower part of the cavity of the body, ascending the gut as two very thin milk-white canals, with irregular windings and twistings, and forming, chiefly at the beginning of the female branch, several convolutions, one of which reached a great way up the female branch and then returned. On their passage, both receptacles decreased towards the middle of the body for a short space, but soon changed into two thick dirty-white tubes, which in breadth nearly equalled the circumference of the intestinal canal. Near the end of the gut these tubes bend, and then turn upwards and reach the place where the male and female branches join, after both having formed some few and small windings: here they are united into one vessel, and send a very thin tortuous canal to the above-mentioned slit. From the construction, order, and contents, I supposed this double receptacle to be nothing less than the female generative organs, such as we find in most of the *Nematoidea*. One could easily distinguish the following parts on it, namely, 1st, The two thin and milk-white vessels, situated in the back part, are certainly the *ovaries*. They contained in their posterior convolutions a fine granular white substance; while in the anterior its granules were more firmly pressed together, and on the ovarium being injured, escape adhering together in a cylindrical form. Nearer to the upper part the granules were clustered together in small heaps, so that the whole matter appeared already to have separated in simple *vitelli*;

none of these heaps of granules possessed a distinct membrane or a clear partition. A bright spot appeared in some heaps, which may probably be the germ*. 2nd, The small narrow pieces of the two vessels which succeed the *ovaria* may be compared with the fallopian tubes. Both vessels were here so narrow, that never more than one grain at a time could pass through the canal, while at the same time four to five such granules were contained in the calibre of the oviducts. By the extension of the two vessels the heaps of granules take at once the form of oval eggs, and we can suppose these two wide canals to be, 3rd, The oviducts or *uterus*. Each egg was enveloped in two delicate colourless tunics. The last egg contained a granular matter, equally divided, which filled out the greatest part of the tunics. I could perceive here no germ; it had, perhaps, retired to the middle of the yolk. If you examined the eggs in the *uterus* which are situated more in the upper part, you find that the granules of yolk matter come nearer to one another, and that they at present do not occupy the whole space of the coverings. Still more anteriorly the granular matter in the eggs begins to separate into several round heaps, which hang closely together under one another, and give the form of the yolk a somewhat ragged appearance. In the interior of each of these granular heaps a clear bright spot was to be seen. Such was the state of the eggs to the uppermost end of the two horns of the *uterus*. After both horns had united into one canal, this had become, 4th, The *vagina*, and so narrow that no more than one egg at a time could slide through it. This *vagina* ended after a small bend into the slit towards the outside, and emitted eggs in my presence.

If we now return to the vessels of the male branch, it might occur to us that the vessel described is the seed vessel, and the two long hard substances the double penis. Truly I am indeed here not able to say in what connection the excretory organs of the testis stands to the female generative organs, whether both possess one opening, or have both different ones, each of which opens externally.

* I have already in this *Archiv*, (I. 1, p. 79.) made observations on this germ in the *Nematoidea*: since then I have seen it quite distinctly in *Spiroptera contorta*, *Ascaris vesicularis*, *lumbricoïdes*, *ensicaudata*, *aucta*, and in *Trichocephalus unguiculatus*; in the last I perceive it even in the ovarium before the yolk was covered with shell. On the contrary, it appears to be more and more confirmed that this bladder is wanting in the eggs of the *Acanthocephala*, *Trematoda*, and *Cestoidea*. Up to the present time I have found no trace of the same in the eggs of the intestinal worms belonging to these orders. Wagner also did not perceive it in *Tænia* and *Distomum*. (See Müller's *Archiv*, II. 4, p. 375.)

On the nervous, vascular systems, &c., I can state nothing, nor do I intend to say that my description of this worm can be called complete; this I hope will be pardoned on account of its rarity, which was the reason that I could not give a better description of it. Perhaps I shall be able at a future time to examine it more thoroughly. The main thing was not to keep this remarkable parasite longer from the friends of helminthology. It is certain that the same worm may be discovered in many other birds, although it is rather curious that three so very different birds as the woodpecker, the swallow, and the hen, should be infested by the same intestinal worm. It belongs therefore to those parasites which are diffused in the same classes of animals, as is the case in *Distomum hepaticum*, *lanceolatum**, and *ovatum*†, &c. That the *Syngamus trachealis* forms quite a new genus of intestinal worms will be clear to every one. By the name *Syngamus* I thought to signify the combination of a male and female individual in one animal. The connection of two individuals of different sex is here also combined with the circumstance that both parts remain much more individualized, as each possesses an intestinal canal, while, in *Diplozoon*, both androgynal halves have one and the same canal ‡.

If we seek a place in the system for *Syngamus*, we find ourselves at a loss, as it comes into none of the five known orders. I am, however, sure that even in a better system there would be a difficulty in placing it, on account of its remarkable properties. If we consider the male and female parts each alone, we find they have a great resemblance in their form and structure to the *Nematoidea*. The body is protended and cylindrical; the intestine is simple, and possesses a muscular *œsophagus*, which is also protended; the generative organs have the same appearance as in many of the *Nematoidea*. The uterus, ovary, &c., are double, as in *Ascaris*, *Spiroptera*, *Strongylus*, and others §. The *ovaries* are not branched like those of the

* Some time ago I found the gall-bladder and gall-vessels of the liver in a young cat filled with many hundreds of this Distomum.

† The *Distomum ovatum*, which till at present was known as only inhabiting the *bursa Fabricii* of the *Corvus Cornix*, *frugilegus*, *Pica*, of the *Anas clypeata* and *Fulica*, (see Rudolphi's *Synopsis*, p. 93,) I have found in the same organ also of *Falco Subbuteo*, *Corvus glandarius*, *Corvus Monedula*, *Turdus viscivorus*, *Hirundo urbica*, *Parus major*, *Crex pratensis*, *Gallinula Porzana*, *Chloropus*, *Uria Troile*, and *Phasianus Gallus*. I also found it several times of enormous size even in the white of hen's eggs. The place of this parasite seems to be supplied in sea birds by *Holostom. platycephalum*, which Creplin (in *Observat. de Entozois*, p. 39,) has described at first from *Colymbus rufogularis*, and which I have taken from a doubtful organ of *Larus canus*, *fuscus*, *Halieus Carbo*, *Colymbus septentrionalis*, and *Falco Albicilla*.

‡ Nordmann's Memoirs, I. p. 67.

§ *Trichosoma* and *Trichocephalus* have only a simple uterus and a simple ovarium.

Trematoda, but simple and manifold tortuous canals, like those we find in all the *Nematoidea*. The *testis* with its bifurcate penis, according to its structure, coincides with the male generative organs of the cylindrical worms. Further, the generative organs lying together with the intestinal canal in one cavity of the body forms another character which belongs to the parasites of the first order. It was only the blind end of the intestinal canal, and the want of rings on the skin, which could cause any difficulty as to its being placed among the *Nematoidea*; much less its wide mouth, which, as before remarked, has more resemblance to the basin-like sucker of the *Trematoda*, yet not formed of a muscular, but of a horny substance. Several species of *Strongylus* show the same. The two before-mentioned basin-formed mouths may have misled Montagu, as he classed this animal with the *Fasciola*; and having given the mouth as being divided into six parts, Rudolphi thought proper to place it by the side of *Distomum lineare* (*poro antico nodulis sex cincto*), as he supposed the male neck with its mouth-opening to be a petiolated *porus posticus*. Thus considered, this worm could not acquire the interest which properly belongs to it; as at the same time a true double-branched *Distomum* could be placed by its side; I mean *Distomum furcatum* *.

I take leave, in conclusion, to call attention to the following comparisons, which, in the examination of this worm, after having learnt its true structure, forced themselves on me, without intending to lay value on them. In all round worms the males are shorter than the females. In *Syng. trachealis* the male portion is by far much shorter than the female. In the male worms the caudal extremity, where the generative organs always terminate, always joins at the pairing-time with the body of the female, forming an acute angle †. Bearing this in mind, we have the following transition to our monstrous form. Both sexes of almost all cylindrical worms unite only at the pairing-time. The male of the *Hedruris androphora* ‡ has the habit at other times also of embracing its

* Rudolphi, *Synopsis*, p. 107, No. 72; and Bremser, *Icones*, tab. ix. figg. 13, 14.

† See in Bremser, *Icones*, tab. iii. figg. 8, 15; and Gurlt, *Lehrbuch der Patholog. Anatomie der Haus-Säugethiere*, tab. vi. fig. 35.

‡ Nitzsch, in Ersch and Gruber's Encyclopædia, Th. vi. p. 49, and Th. ix., 3rd taf., fig. 7; also in Schmal, *Tabulæ Anatomiam Entozoorum illustr.*, tab. xvii., fig. 5. Nitzsch observed (p. 49,) at the margin of the caudal socket of the female, a small arm which he thought to be the undeveloped tail end. I have satisfied myself that this arm is of a horny nature, and that, as it enters into the membrane of the stomach, it serves as a firm hold for the female worm. The place on which the animal fixes itself is also perceivable on the outer side of the stomach by a tuberculous projection.

female; here also a continued junction of both genders without being grown together, and in *Syngamus trachealis* a continual combination, by being really grown together.

Explanation of the Figures (*Plate* I.).*

Fig. 5. *Syngamus trachealis*, natural size.

Fig. 6. The same (another specimen, with much longer male branch) greatly magnified. *a.* Male branch. *b.* Female branch. *c.* Incision into which the generative organs descend.

LII. *On the Muscular Effort required to ascend Planes of different Inclinations. By Professor* Forbes.†

IT has been long pretty generally known that the same expense of muscular energy will carry a person through nearly the same vertical height in a given time, at all angles of elevation not very small. At least we find writers on the subject of strength, measuring effort by the number of pounds raised multiplied into the height attained. I am persuaded that this is much more rigorously true than is usually supposed, and that a person in uniform health and of a good habit of body will ascend through almost precisely the same number of vertical feet in an hour (unloaded) whether the ascent be rapid and short, or tortuous and little inclined, excluding, of course, extreme cases.

All paths by which mountains are usually ascended on foot, lie within these practical limits; the *mean* angle of ascent on footpaths varying in different cases from 10° to 25°, seldom being below the former, and almost never exceeding the latter. In fact, the angle of ascent is determined, not so much by the configuration of the ground as by the instinctive (or rather experimental) modification which the walker gives to it by such zigzag courses as increase the length and diminish the steepness of the path. This makes the difference of ascent less than might at first be supposed. Thus the time required to ascend a mountain whose height is known, may be pretty accurately predicted by any individual who has determined the measure of his muscular energy; and conversely, heights may be deduced from the times of ascent with surprising accuracy, a result of great use when more exact methods cannot be employed.

From the observation of Borda upon the men who accompanied him in ascending the Peak of Teneriffe, the ascensional effort was 1225 English feet per hour, continued for eight

* The Plate was given in the Number for March.

† Communicated by the Author.

hours in a day. This estimate does not differ much from that of the guides at Chamouni. My own experience would give results somewhat greater, and the *constant* must of course be determined by each individual for himself. For heights not exceeding 5000 or 6000 feet I find 1500 feet per hour to correspond to the most moderate sustained muscular effort, and which might be long kept up; the only kind of observation of any value, since excessive exertion puts all calculation at defiance.

I long since proposed to myself to find an expression for muscular exertion in walking which should include inclined planes of all inclinations, and consequently a horizonal road as a particular case, where the preceding reasoning becomes inapplicable, since the distance corresponding to a given height is infinite. During the prosecution of experiments at great elevations already partly before the Royal Society of Edinburgh, I have had an opportunity of extensively considering this practical question. My practice when in Switzerland in 1832, was to measure from time to time the inclination of the path with a klinometer, and to note the times elapsed, making observations when necessary for the total height with the barometer. The continued and equable ascent of some of the carriage passes of the Alps afforded a good opportunity of observing at small inclinations when the horizontal component became relatively great, whilst long experience gave me perfect confidence in the uniformity with which I walked day after day, at the rate of four miles an hour, on level ground. These experiments seem to indicate about 700 vertical feet of ascent per hour at an angle of $3\frac{1}{2}°$, from 1000 to 1100 feet at 7°, and nearly 1500 feet per hour from 10° to 20°, without much variation, but rather indicating that the higher angles were the more advantageous.

For inclinations greater than 25°, there is a deficiency of directly comparable observations. Coulomb could not persuade a labourer to ascend a stair cut in the rock, through a height of 9000 feet in a day, at the usual wages. The angle may be supposed equal to 30° or 35°. This may be considered to indicate a diminution of useful effect, and confirm the existence of a maximum advantage between 20° and 30°.

At very high angles the tread-mill affords some curious results. It appears from the statistical tables* that though the velocity of tread-wheels varies much, as well as the total work done in a day, yet there is more uniformity in the hourly

* See Parliamentary Reports, and particularly Prison Discipline Society's Report for 1823.

vertical ascent than might be expected. The reliefs being more frequent when the velocity is great, the work done varies in summer and winter, but may generally be stated at from 1400 feet per hour.

The most remarkable circumstance is the high angle of elevation at which this effort is performed, and which I doubt not might be advantageously modified if œconomy of the labour of prisoners should ever become an object. I have examined the operation of the tread-wheel in Edinburgh, and find that the angle of ascent is about 80°, and sometimes almost vertical. It is not to be supposed that this angle is as favourable as a smaller one, though the work done is greater than in some cases already considered; this being a punishment and considered as hard labour, the other (the ascent of mountains) being only equivalent to an active man's common daily work. The view in making the angle of ascent of tread-wheels so great is, no doubt, to secure the greatest effective leverage of the man's weight; but by duly increasing the radius this leverage might be attained at any other angle, and the stepping-boards instead of being prolongations of the radii should form an angle with them such as to make them horizontal at the part of the wheel where the moving power is applied, which should be when the radius forms an angle perhaps not greater than 40° with the vertical.

The tread-wheel observations are the most satisfactory at high angles, since when ladders are used (as in the Cornish mines, and those which communicate with the village of Albinen near Leuk in the Valais,) powerful assistance is afforded by the arms.

In the want of decisive observations, we shall assume 1100 feet per hour as the ascensional effort at the extremity of the scale when the ascent is vertical, or the horizontal component null; being somewhat more than two thirds of the maximum effect. This must be considered, however, as in some measure conjectural, and to be corrected by future experience. I would also add, that it is distinctly to be kept in view that the effort which we mean to represent is not an extraordinary but an ordinary due; not what by a certain degree of exertion might be accomplished for a single day, much less the maximum effect attainable, but an amount of labour which may be continued for eight hours aday at least, and for many days together*.

Since we propose to include in a formula both *distance* and

* As an example of the necessity of attending to this, and also of the usual method of observing, I quote my observations during the ascent of the Rigi, a hill of comparatively small height, and particularly accessible:

height, as they respectively exhaust muscular power, that empirical formula ought to be such a function of α, the angle of inclination, that, 1, When $\alpha = 0$, the distance shall be four miles nearly, and the height ascended in an hour $= 0$; 2, When $\alpha = 90°$, the height ascended, or *distance* $\times$ *sine* α, shall be $= 1000$ feet nearly; 3, When α is between 20° and 30°, the ascensional effort or dist. $\times \sin \alpha$ shall be a maximum, and $= 1500$ feet nearly; 4, That it shall vary slowly between 10° and 30°, and that below 10° it shall adapt itself nearly to the experiments.

Now, a formula of this kind,

$$h = \left\{ \frac{a}{\sin(\alpha + \theta)} - b \sin \alpha \right\} \sin \alpha,$$

sufficiently well satisfies these conditions; h being the vertical height in English feet attained in an hour, a and b being constants, and θ a constant angle. It will at once be seen that the part within brackets measures the *distance* traversed, and determines the very rapid rate of diminution of the expression for the first degrees; a fixes the horizontal distance passed over in an hour, and b the diminution of ascensional power when the inclination is nearly vertical. Should this diminution be much smaller than we have supposed, the second term might be independent of α, and should the maximum effect be at a vertical inclination (which however is most improbable) it would disappear altogether. The following numbers seem best to satisfy my experiments:

$$h = \left\{ \frac{1900}{\sin(\alpha + 5°)} - 800 \sin \alpha \right\} \sin \alpha \text{ in English feet.}$$

The following table exhibits the results of this law:

Angle of Ascent.	Space described in an hour.	Height ascended in an hour.
0°	21800 feet.	0 feet.
5	10872	948
10	7202	1251
15	5348	1384
20	4198	1436
25	3462	1463
30	2913	1456
50	1706	1307
70	1215	1142
90	1107	1107

the result it will be seen is greatly above the mean effect. "1832. Oct 15. Left Weggis $2^h\ 5^m$. Angles of ascent 9°, 15°, 15°, 8°, 11°, 16°, 11°, 15°, 2°, 12°. Mean 11°·4 Rigi—Culm. $4^h\ 35^m$" The height is 4400 English or at the rate of about 1800 feet per hour.

LIII. *On the Aurora Borealis of February 18th, 1837, as observed at Sidmouth, in Devonshire. By* N. S. Heineken, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THINKING that some of your readers may feel an interest in a subject which is becoming of much importance in meteorology, I am induced to forward, for insertion in the Philosophical Magazine, the following account of some observations which I made during the occurrence of the most splendid aurora borealis which I have ever witnessed.

I am, Gentlemen, respectfully yours,

N. S. Heineken.

At 16^{m} past 7 (mean time), in the evening of the 18th of February (Saturday), I observed the commencement of an aurora borealis of the most vivid pink hue, occasionally approaching to scarlet. At this time the centre of the brightest part of the aurora bore, by compass, about 6° north of west, allowance being made for variation. At 27^{m} past 7 the bearing continued nearly the same. The altitude of the upper edge (which was not well defined), taken, as near as might be, at the time with a Gunter's quadrant, and verified afterwards with a reflecting sextant, was about 55°. At 32^{m} past 7 the former bearing of the main body continued the same, but the tint had now spread round to the N.E. The altitude of the brightest part was from 35° to 36°, the altitude of the edge remaining much as before. The stars in Cassiopeia shone through the aurora (though not the most brilliant part of it) somewhat more dimly than usual, and they *appeared* to partake of the colour also, as did other stars. The thermometer stood now at 43½°, the barometer at 29·566. The *smallest lock* of white cloud (cirrus?) passed over at 3^{m} before 8 to the eastward: at 15^{m} past 8 fleecy clouds arose from the very point of the aurora's first appearance and passed to the eastward. The gold leaves of the electrometer were now *slightly*, but evidently affected. I mention this latter circumstance (which I observed again in the course of the evening) with some hesitation, finding that upon several occasions in the Polar expeditions no such indications were given. The wind, I may mention, was now very moderate. About 9^{m} before 9 the aurora shifted to S.S.W.; a slight pink band appeared between Rigel and Sirius; an arch was formed of the same colour at 1^{m} before 9, edges not well defined, passing between Aldebaran and η Orionis, over Castor and Pollux, and extending a little beyond them on either side, and, still continuing, the western-

most edge was bounded by β and γ Ursæ Majoris. The arch appeared to arise from a mass of vivid pink light, but obstacles prevented me from seeing the terminations. Arch again visible at 4^m past 9, nearly as before. At 12^m past 9 the thermometer had fallen to 41° and the air was sensibly cooler; several faint white streamers shot up in magnetic north; no appearance at 17^m before 10. A most splendid arch at 8^m past 10 passing between η Orionis and Aldebaran; one edge bounded nearly by Castor, and the other by ϵ and γ Ursæ Majoris. Upon going indoors to an upper window I could see this magnificent arch, more glorious even than the arch of promise, terminated on each side by the hills between which Sidmouth is situated, and attaining an altitude of, I should suppose, between 80° and 90°. At each termination the mass of light was wider, at least twice the width of the higher parts of the arch, and in that to the eastward I observed two or three well-defined dark vertical spaces or bands. At 18^m 30^s past 10 several white streamers, which I could not discern out of the arch, shot across it from the western side near Capella. The streamers, which had the appearance of white bands, made by rough estimation an angle of 60° or 65° with the western edge of the arch. At 11^m before 11 patches of irregular full pink light had taken the place of the arch; a large brilliant mass being over Ursa Major, and to the southward of it. Many fleecy clouds again arose from the point of the aurora's first appearance. The only meteor which I observed during the evening was at 20^s past 11, brilliant, but not large. It appeared (by estimation) about 2° 30′ above Capella; extent, of course to the north-westward, 5° or 6°: duration little more than a second, leaving no train. After this period the aurora became fainter, and finally ceased.

I may observe that the day had been stormy, with heavy showers, the wind in squalls from S.S.W. A short time before the appearance of the aurora the wind lulled, and, except during the times I have noted as cloudy, the moon was shining brilliantly during the whole of this, notwithstanding, most splendid spectacle. I had thus also afforded to me an opportunity of correcting my watch by observing the occultation of Mars. I have to regret that I had not completed a needle for magnetic observations. I subjoin, however, the state of the thermometer and barometer on the morning and evening of the 18th and 19th.

	A.M. 9 o'clock. Therm. Max.	Min.	Bar.	10 P.M. Therm.	Bar.	
18th.	50	42	29·768	40	29·610	
19th.	48½	37½	29·480	49½	29·068	Stormy—

damp—showers; and until today (25th) the weather has been very stormy, with heavy wind, rain, and hail.

Sidmouth, February 25, 1837.

P.S. In the Magazine for last month, (January,) p. 75, Mr. Mallet has mentioned the occurrence of an aurora on the evening of October 5th, 1836. May I be allowed to state, that at about 7 o'clock on that night I observed, at Sidmouth, a patch of faint pink light partly over U. Major, but there was no further appearance of aurora here?

LIV. *Experimental Researches on a peculiar Action of Iron upon Solutions of some Metallic Salts. By Dr.* C. F. Schœnbein.*

SOME time ago I published several papers†, in which I made known some very remarkable facts regarding the action of iron upon oxygen. According to the notions generally adopted by philosophers respecting the action of metals performing the function of the positive electrode upon oxygen set free by voltaic action, iron, as one of the more readily oxidable metals, chemically combines with that element. In one of the papers alluded to, I have shown that these notions with regard to iron do not hold good in all cases, and that this metal acquires under certain circumstances the property of platina or gold, that is to say, that whilst constituting the positive electrode, it is neither oxidized nor otherwise chemically affected by oxyacid solutions, which usually act upon iron with more or less violence. I have further observed, that this inactivity of iron depends upon the manner of closing the circuit, as well as upon the chemical nature of the electrolytes contained in the solutions in which the polar wires of the pile are immersed. Solutions containing oxyelectrolytes which act chemically upon iron, as, for instance, sulphuric or nitric acid, require the circuit to be closed in a certain manner in order to evolve oxygen at the positive iron. Solutions containing oxyelectrolytes which do not sensibly act upon iron, as, for instance, those of potash, soda, and a great many oxysalts, allow the evolution of oxygen at the positive iron quite independently of the manner of closing the circuit. In solutions containing, besides oxyelectrolytes, others of a different nature, for instance, hydracids, haloid salts, &c., no evolution of oxygen takes place in whatever manner the circuit may be closed. From these facts, and others stated elsewhere, I am

* Communicated by the Author, through Mr. Faraday.

† See Lond. and Edinb. Phil Mag., vol. ix. pp. 53, 122, 259; and present volume, p. 133, 172.—Edit.

inclined to infer that the affinity of iron for oxygen is destroyed by a current moving through the metal in a certain direction, and that the affinity lost in this way by the iron is revived by an opposite current. To ascertain whether this view holds good generally with regard to iron, I have made a series of experiments, the results of which are as follows. I introduced an iron wire which had previously been connected with the positive pole of a small battery, into an aqueous solution of the common sulphate of copper, by means of which the circuit was closed. According to my hypothesis no action whatever is to take place on the part of the iron upon the solution, under the circumstances here stated. My expectations were fully realized, for after many hours' action of the pile not the smallest particle of copper was deposited on the iron wire, its surface had not undergone the least change, and during the whole time of action oxygen was evolved at the iron. But as soon as the passage of the current through this metal was opposed only for a moment, for instance, by taking out of the copper solution either the negative polar wire or the positive one, there appeared on the surface of the latter a film of copper. (The same result was obtained by joining momentarily the two polar wires within the solution, or by touching the iron wire with another metal capable of precipitating copper.)

Now, by these facts I think two things are clearly shown, first, that iron ceases to have any affinity for the oxygen both of the oxide of copper and of the water decomposed by voltaic action, and secondly, that this state of chemical indifference lasts only so long as there is a current passing from the iron into the copper solution. This influence of current electricity upon the chemical bearings of iron is highly interesting, not only on account of its being contrary to the electro-chemical notions hitherto entertained on the subject, but also on account of the circumstances under which the oxygen resulting from the decomposition of water is presented to iron. These circumstances are, indeed, of such a kind as highly to favour the oxidation of the metal, for oxygen in a nascent state is brought into contact with iron, and there is at the same time a portion of the acid of the salt set free by voltaic action at the iron wire, which also tends to occasion the oxidation of the latter. The same remarks apply to the fact already stated, that iron is not acted upon by nitric or any other oxyacid, provided this metal is placed under the influence of the pile in the manner above mentioned. Iron, however, may acquire the property of being not acted upon by nitric acid or solutions of certain metallic salts without being sub-

jected to the influence of a current. This remarkable fact has been observed by Sir John Herschel, and more recently by Mr. Faraday and myself. If, for instance, a common iron wire, having been made inactive by repeated immersions in common nitric acid, is put into a solution of blue copper vitriol, not the least chemical action takes place. It is true that it happens sometimes that such a wire precipitates copper at the moment of its being plunged into the solution, but in such a case the inactive state of the metal had ceased previously to immersion. Now whether iron is or is not in its peculiar condition may easily be ascertained by the appearance of the surface of that part of the wire which had been immersed in nitric acid. If the surface is bright, the wire is inactive; if yellowish brown, the metal has assumed its common state, and will consequently act upon the copper solution in the usual manner. The peculiar condition of iron with regard to the copper solution can be destroyed in different ways. In the first place it may be destroyed by making an inactive iron vibrate. If such a wire is wetted by the said solution, and afterwards rather violently struck against any solid body, for instance against a table, immediately after the shock a film of copper will make its appearance along the whole wetted surface of the wire*. According to the results of my experiments, published in several periodical works, inactive iron is rendered active with regard to nitric acid by the same means. In the second place the active state of iron may be reproduced by touching the inactive metal with a metal which acts chemically upon the solution of the copper salt. If an inactive wire is wetted by this solution, and then touched, on any point of the part wetted, with a piece of common iron, zinc, cadmium, tin, lead, arsenic, or even copper, the precipitation of copper instantaneously takes place at the point of the iron wire where contact had been effected, and this action rapidly extends itself over the whole part of the wire which is covered with the solution. It is a matter of course that the same effect can be obtained by touching the inactive iron wire within the solution of the copper salt with the same metals. But the peculiar condition of iron may be changed into the common state without immediately touching those parts of the metal which are surrounded with the copper solution. If, for instance, an inactive wire is put into the solution so as to allow part of it to rise above the level of the fluid, and if a wire of any of the

* This remarkable fact, considered by itself at least, tends to confirm Prof. Faraday's impression as to the cause of the peculiar voltaic state of iron.—Edit.

metals above mentioned is placed in the same solution, having likewise one of its ends raised above the surface of the liquid, copper will be precipitated as soon as the free ends of both wires are made to touch one another. This mode of changing the state of iron is exactly the same as that by which a similar change of condition of this metal may be effected with regard to nitric acid. Now, all these facts evidently prove that the peculiar condition of iron, whatever the cause of it may be, is always destroyed by the chemical action of metals brought into contact with iron when in the inactive state. There is certainly one singular fact, which seems to indicate as if contact independently of and unconnected with chemical action could of itself occasion a change of state in iron. It has been already stated, that copper brought in some of the ways mentioned into contact with an inactive iron wire, which is immersed in the copper solution, renders the latter metal active. Now copper of course cannot be precipitated from the solution of blue vitriol by copper; the chemical action of this metal upon the copper salt must, therefore, be essentially different from that which is exercised by the more readily oxidable metallic bodies in question. First, I thought there might, perhaps, be some free acid contained in the solution, and by this means chemical action occasioned. To ascertain the correctness of this view, I added ammonia to the solution until flakes of oxide of copper were beginning to make their appearance; but the copper wire acted in such a neutral solution in the same manner as it did in the more acid one; chemical action consequently does not result from the cause supposed. I think there is only one way left to account for the fact in question. It is well known that copper put into a solution of a salt containing the deutoxide of this metal, unites by degrees with this base, to form protoxide of copper. Although this chemical action is extremely slow and weak, still it is of sufficient power to revive in the inactive iron its dormant affinity for oxygen.

There is no doubt that, one case excepted, in all others hitherto mentioned, in which passive iron is rendered active, an electric current is produced, passing from the metal in which chemical action originates, through the solution, into the inactive iron, and from this back again to the first metal. It is further obvious, that the direction of the current passing through the inactive iron is opposite to that in which the current moves through an iron wire which performs the function of the positive electrode of a pile. The chemical effects produced upon iron by these different currents being also the reverse of one another, it seems to me that these facts

speak in favour of the idea already suggested that the chemical affinity of iron for oxygen is destroyed by one kind of current, and called forth again by the other. It is true, one of the most sagacious philosophers of the age, Mr. Faraday, has started an idea which seems to account very satisfactorily for the phænomena in question. According to his view the peculiar condition of iron depends either upon a film of oxide covering the metal, or upon a relation of oxygen to iron equivalent to oxidation, so that the particles forming the surface of the inactive iron have satisfied in one way or other their affinity for oxygen. Applying the same hypothesis to account for the bearing of iron in the solution of blue vitriol which Mr. Faraday has made use of for explaining the singular action of iron upon nitric acid, we must say, an inactive iron wire does not act upon the solution of the copper salt, because there is no immediate contact between the truly metallic particles of the wire and the said solution, on account of the interposing film of oxide, or something similar to it. But if now another metal be put into the solution, which is chemically acted upon by the latter, there is a current produced, proceeding from the active metal, and passing through the solution into the inactive iron. By this current water is decomposed, hydrogen evolved at the iron; the film, or what is equivalent to it, deprived of its oxygen; and by this means a truly metallic surface of the iron wire produced. Though this way of accounting for the facts in question recommends itself by its beautiful simplicity, and what is still more valuable by the great advantage of bringing back an apparently anomalous case to a general law, still there are weighty reasons stated by me elsewhere, which will hardly allow the adoption of Mr. Faraday's sagacious hypothesis.

After having examined the action of iron upon the common sulphate of copper, I was curious to see how the same metal acts under similar circumstances upon the solutions of the nitrates of mercury. Before entering into details upon the subject, I must not omit to state, that I did not observe any essential difference of action between the protonitrate and pernitrate of mercury. A common iron wire, cleaned or not, when put into a solution of either of the neutral nitrates of mercury, does not act in the least upon the salt, that is to say, no mercury is precipitated on the iron; but what is still more surprising, the iron wire, after having been immersed only for a few seconds in such a solution, shows all the properties of inactive iron; it will, for instance, not be acted upon by common nitric acid, nor by a solution of blue vitriol. Even when a strong solution of the mercurial salt is diluted with 1000

times its volume of water, it will still render an iron wire inactive, though in this case, as might be expected, some time is required for obtaining the effect. But if a common iron wire is first immersed in water containing so little of nitric acid as scarcely to change the colour of blue litmus paper, and afterwards plunged into the solution of mercury, it will precipitate the latter metal. It is, indeed, quite extraordinary how far this influence of the acids, favouring metallic precipitation, extends. I mixed a strong solution of neutral protonitrate of mercury with 1000 times its volume of water, and in the same proportion I diluted common nitric acid. By putting the wire first into the acidulated water, it always acquired the property of decomposing the diluted solution of mercury on being plunged into it. Common muriatic acid, even 4000 times diluted with water, produced the same effect. Though it is a well-known fact that some free acid contained in metallic solutions favours the precipitation of one metal by another, still I am not aware that any chemist has as yet stated any particulars regarding the extent and cause of this influence. The peculiar action of acids mentioned seems to be intimately connected with the subject of my researches on the action of iron upon nitric acid, and to afford a case similar to that presented by inactive iron in its bearing to strongly diluted nitric acid. In one of my published papers on the subject, I have stated that inactive iron loses its peculiar condition by being put into diluted nitric acid; the same thing takes place in the case before mentioned. Common iron wire is of itself inactive in a solution of a neutral salt of mercury, but is rendered active by being subjected to the action of acidulated water previously to its immersion in the solution. According to Mr. Faraday's views the acid must produce the effect spoken of by cleaning the surface of the wire, that is to say, by dissolving some film, with which even a common wire must be supposed to be covered; but for reasons already alluded to, I cannot entertain the opinion of this distinguished philosopher, even in this case.

The view I have taken of the subject leads me to ascribe the effect in question to chemical excitement in the metal occasioned by the acidulated water. As iron having only for a few moments been immersed in diluted acid decomposes the neutral solution of mercury, it might be supposed that this metal should act in the same manner in a solution made somewhat acid. But I found this not to be the case. A solution of pernitrate of mercury obtained by saturating nitric acid, sp. gr. 1·35, with peroxide of mercury, was mixed with its own volume of the same acid. A common iron wire put into

this acid solution had no action upon it, and assumed its peculiar condition. I could put even twenty volumes of nitric acid to it, without producing any action. But I must not omit to state the singular fact, that there is in this respect a great difference between a wire which is cleaned and one which is not. If, for instance, a common iron wire has only once passed through a piece of linen or cloth, it will be acted upon by the acid solution containing only one volume of nitric acid, whilst an uncleaned one is not affected at all. This difference is the more remarkable, as an uncleaned wire is much more violently attacked by mere nitric acid than a clean one. Another fact, still more singular, is, that different parts of the same piece of an uncleaned wire are sometimes differently acted upon by the same acid solution of mercury, one part being, for instance, entirely inactive, whilst another contiguous to it proves to be highly active. I call this fact a very singular one, because every bit of a whole roll of iron wire is acted upon in common nitric acid. When an iron wire cleaned or not is plunged into the solution of mercury containing from 30 to 50 times its volume of nitric acid, it will be affected, and continue to be acted upon if left in the solution; but when it is again taken out of the fluid and held for hardly a second in the air, after its reimmersion it will prove entirely inactive. It is surprising, that almost the same results are obtained at very different degrees of temperature. I heated a mixture containing 20 volumes of nitric acid and one volume of the solution of pernitrate of mercury to its boiling-point. The end of an iron wire put into it was certainly acted upon, but by withdrawing it only for a few moments from the solution it was rendered inactive, so that it could afterwards be reimmersed in the nearly boiling acid fluid without being attacked by it. A certain proof that the metal acquires, even at this high degree of temperature, its peculiar inactive state is, that when put into a solution of blue vitriol, or into mere common nitric acid, it does not in the least act upon these substances. In making these experiments I frequently observed the curious fact, that the iron wire immersed in the nearly boiling acid solution loses its inactive condition as soon as it is a little raised so as to expose to the air a very small part of that portion of wire which has been immersed in the fluid; but though this is often the case, it is not invariably so.

The results which I have obtained from experiments made with iron wire and an acid solution of mercury much diluted by water, are likewise worthy of being stated. One volume of a very strong solution of neutral protonitrate of mercury, five

volumes of nitric acid, sp. gr. 1·35, and 200 volumes of water were mixed together. A piece of cleaned iron wire put into this solution did not precipitate mercury. By plunging such a wire into water slightly acidulated, its power of acting upon the salt of mercury, as above mentioned, is instantaneously called forth. The wire having once acquired this power retains it; provided, however, it be called into play at intervals of time not much exceeding a second or so. But if the wire after having been active in the solution is taken out of it, cleaned from the adhering mercury, and left exposed to the air only for a few seconds, it will have lost its property of precipitating the last-named metal, and rest entirely inactive in the solution, whatever length of time it may remain immersed in it. This remarkable and sudden change of the condition of iron is most likely due to some action of the air; for if the wire, being still in its active state with regard to the solution of mercury, is put into water or hydrogen gas, it preserves its precipitating power. I have not yet put iron into other mediums than those mentioned, nor have I examined whether moisture has anything to do with the phænomenon. At any rate this subject seems to me in many respects sufficiently interesting to deserve further investigation. Before passing to another subject, I have still to mention some facts connected with those just spoken of. An iron wire which proves to be entirely inactive in the last-mentioned solution of mercury, is not so with regard to a solution of blue vitriol or to common nitric acid; for a wire which does not throw down mercury, precipitates copper, or is violently acted upon by the said acid. From chemical reasons we are led to expect that the very contrary should take place, the affinity of copper for oxygen being much greater than that of mercury; that is to say, we should think the mercury salt easier to be decomposed by iron than the copper salt. It seems, therefore, as if the anomalous fact does not result from the action of common affinity. Another fact worthy of remark is, that iron acts quite differently upon the neutral nitrates of mercury dissolved in alcohol or æther, from what it does upon the aqueous solutions of the same salts. In the former case iron always precipitates mercury and never turns inactive, whilst, as above stated, in the latter case the contrary takes place. If an iron wire, having been rendered inactive by immersion in an aqueous solution of the mercury salt, is put into alcohol or æther containing the same salt, it loses its peculiar condition and returns into its active state.

I think it not quite irrelevant to the subject treated of in this

paper, if I produce a new case bearing evidence in favour of the theory*, according to which voltaic electricity is due to chemical action. It is true that the beautiful researches of Mr. Faraday, as well as those of Mr. De la Rive, have led to results which remove from an unbiassed mind even the shadow of a doubt on the subject, and which prove in the most satisfactory manner, that mere contact of heterogeneous metals is not capable of disturbing their electric equilibrium. Still, as the number of philosophers is as yet rather considerable who maintain the hypothesis of Volta, I think it not quite useless to increase the body of evidence against it.

If an iron wire rendered inactive by immersion in nitric acid is associated with a platina wire, and two of their ends put into a solution of blue vitriol, not the smallest quantity of copper will be precipitated on the platina; but if the inactive iron is thrown into chemical action by being touched within the solution, either with a common iron wire or by any other metal which chemically acts upon the copper salt, at the very moment of contact a film of copper makes its appearance on the platina. Now if, according to the views of Volta, electricity be excited by the mere contact of different metals, in the case in question a current should be produced, and in consequence of such a current chemical decomposition should take place, that is to say, copper should be eliminated at the platina. But from such not being the case, it follows that there is no current, consequently no electricity, produced by the contact of iron and platina. By having recourse to the galvanometer, the absence of a current under the circumstances mentioned is placed beyond doubt. If the inactive iron wire is connected with one end of the wire of the galvanometer, the platina wire with the other one, and if the two free ends of the iron and platina wires are plunged into a solution of blue vitriol, not the least deflection of the magnetic needle takes place; but as soon as the part of the inactive iron wire immersed in the solution is touched with a metal capable of causing chemical action, the needle becomes agitated, and at the same time a deposition of copper takes place on both wires. From this fact it appears that the oxidation of iron has no sooner been occasioned than two effects of a current are produced; chemical decomposition of an electrolyte, and affection of the needle. Now as previously to oxidation no such effects are obtained, we are fully entitled to draw the inference, that the phænomenon of oxidation bears to

* See Mr. Faraday's conclusive proof, also drawn from the relation of iron and platina, Lond. and Edinb. Phil. Mag., July 1836, p. 60.—Edit.

that of a current the relation of cause to effect, or, generally speaking, that voltaic electricity is due to chemical action and by no means to contact.

I am quite confident that inactive iron can be used in a great number of cases for obtaining results similar to that just spoken of, and that the peculiar state of this metal offers to philosophers in many other respects a most valuable means for making electro-chemical researches.

Bâle, October 1836.

LV. *On the peculiar Voltaic Condition of Iron.* *By* H. M. Noad, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

The singular phænomena presented by iron when put under certain circumstances into nitric acid, which have been so well described in your Magazine by Professor Schœnbein, and for which Dr. Faraday has, with his usual ability, offered an explanation, had such an interest with me that I was induced to repeat all the experiments which have been described in your Journal; and in the course of these I was led to make other observations, which if you think worth while I shall feel obliged by having inserted in your next publication. When a wire that was made inactive by platina was dipped into a vessel of nitric acid, sp. gr. 1·374, and connected with the galvanometer, and a common iron wire, *first* connected with the galvanometer, and then dipped into the acid, no action either electrical or chemical took place; but if it was *put into the acid* first there was always strong action, and the needle was deflected in the same manner as if the second wire was zinc and the first platina; action was then generally communicated for a moment to the first wire, and afterwards both wires were brought to the peculiar state and the needle was of course quiescent: if now either wire was touched in the acid with a common iron or copper wire, it was thrown into action, and the galvanometer was affected, the active wire playing the part of zinc. If instead of an inactive wire in this experiment a piece of platina was used, the moment the circuit was closed and the second wire was in action, bubbles of gas made their appearance on the platina; and if a common iron wire, round which a small piece of platina foil was wrapped, was substituted for the platina wire, these bubbles rose rapidly from all parts of the foil, but not one appeared on the

iron; but when the foil was slipped off from the iron the gas then rose from the iron and continued to do so, but the metal was not thrown into action.

When two glasses were filled with acid and connected by a compound platina and iron wire, the platina or inactive iron in one glass exerted a protecting influence on the iron in the other, provided the communication was first made through the galvanometer: a touch, however, with a common iron wire threw the metal into action, producing a strong electrical current. The same was the case when three or four glasses connected by a compound platina and iron wire were employed.

But the most curious fact that I observed was this: the nitric acid was diluted with water till it had a sp. gr. of 1·204. In this acid iron was not protected by platina even when coiled thickly round it; on the contrary, it appeared to me that oxidation took place with increased rapidity when the platina was in connection. Neither was the iron protected when the connection between the metals was made through the medium of the galvanometer, provided that the iron was dipped into the acid *first*; but if the metal was *first* connected with the galvanometer and then put into the acid, *no action whatever ensued in any length of time*, even after the platina was removed: but once touching it with another piece of iron always threw it into action, it becoming instantly covered with a brown nitrate of iron; the wire thus made inactive did not possess the power of rendering other wire so, but was always thrown itself into action when common iron wire was substituted for the platina, whether it was connected with the galvanometer *first* or not. The first wire in this case acted as platina and the second as zinc with regard to the electrical effect that was for the moment produced.

When two cups were used in this experiment and the connection between them made by a bent common iron wire the result was the same, the platina in one cup protecting the iron in the second. The conditions before mentioned being observed, if now the inactive wire was made active, electrical action was produced, the current being conveyed across the connecting wire. Things being in this state, if the connecting wire was removed electrical action ceased; and if a fresh wire was bent, and one end *first* dipped into the cup containing the active wire, and then the other end put into the cup containing the platina, *that end was immediately in the peculiar state.* Now here there was no metallic connection whatever between this wire and platina, still it was preserved inactive, and there was also no passage for the electrical current, for the needle of the galvanometer was quite still; but when by touching with

a copper or iron wire it was made active, then there was passage for the current, and the needle was strongly deflected. From this it appears that when iron is in the peculiar state it is *incapable of conducting a current of voltaic electricity.* I have one more remark to make, which is, when the iron wire was *inactive* it was found impossible to make either end of the connecting wire so; and if the platina was removed from its cup and a common wire put in its place, it always made the wire in the other cup active.

I forbear to make any observations on the cause of these remarkable phænomena, as the matter is already in such able hands that it would be presumptuous for me to attempt to offer an opinion; trusting, however, that what I have noticed may be the means of inciting further researches,

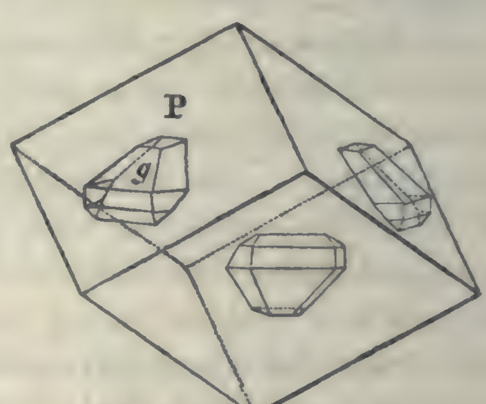
I am, Gentlemen, yours, &c.,

Shawford, near Bath, March 14, 1837. HENRY MINCHIN NOAD.

LVI. *On the Intersection of Crystals belonging to different Minerals in a regular and constant manner. By* H. J. BROOKE, *Esq., F.R.S., &c.**

ON examining lately some specimens of Chabasie from Ireland, I have observed several crystals penetrated by crystals of Gmelinite, with their axes in all instances parallel to those of the rhomboids of Chabasie, and the planes also corresponding in position with those of the Chabasie, as shown in the annexed figure. Sometimes the face of the Chabasie is covered by a single crystal of Gmelinite, and sometimes it is studded with many small ones.

The inclination of the plane P of Chabasie on the axis is 38° 34′, and that of plane *g* of Gmelinite 50°, whence the inclination of *g* on P is 11° 26′. This position is constant in all the crystals I have seen.



I am not aware of any analogous fact having been before noticed, and on looking over my own minerals, I observe only two other instances of the same kind.

One is the combination of Oligiste iron and Rutile. If we suppose a summit of a rhomboid of Oligiste iron replaced by a triangular plane at right angles to the axis, and crystals

* Communicated by the Author.

Fig. 1.

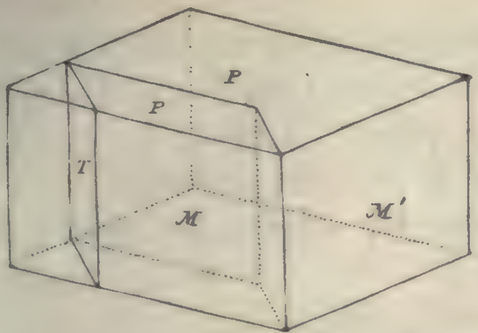


Fig. 2.

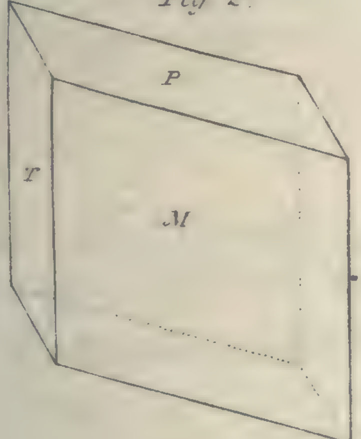


Fig. 3.

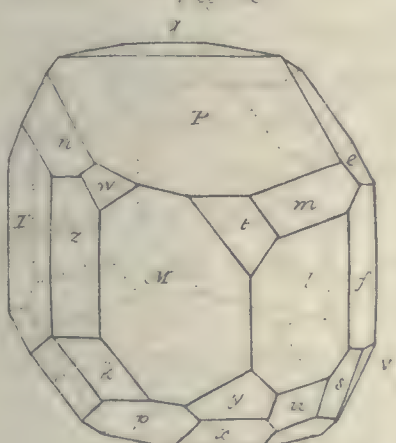


Fig. 4.

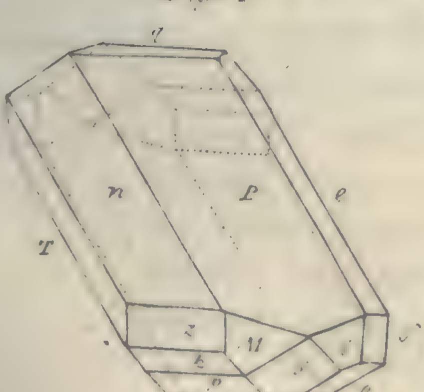


Fig. 5.

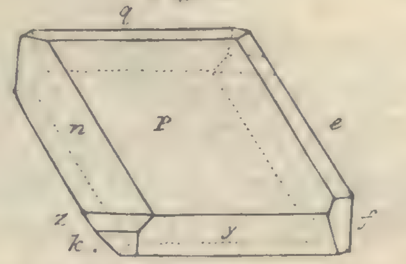


Fig. 6.

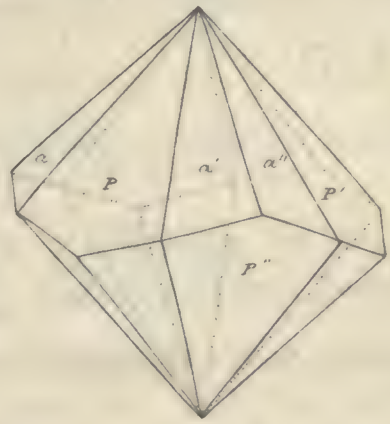


Fig. 7.

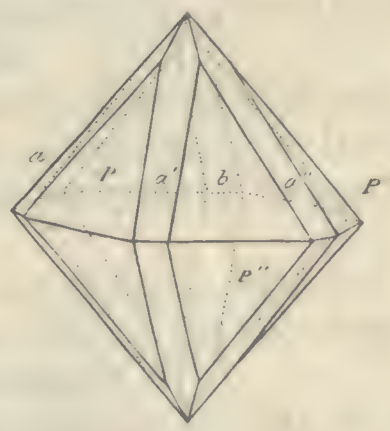
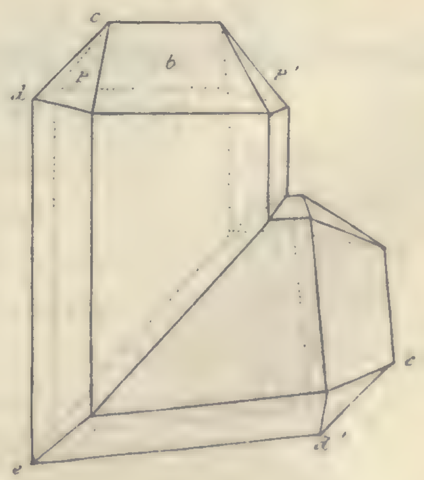


Fig. 8.



F. Brookes Sc.

of Rutile to be imbedded in this plane, they will lie in directions perpendicular to its three *edges* and to the axis of the rhomboid.

The other case is the regular coating of some of the crystals of felspar from Bavaria with cleavelandite. The coated planes are the M, *z*, and *l*, of Häuy, or the M, K1, and 2, of Phillips, and the crystalline striæ of cleavelandite lie in lines parallel to the edge between M and L of Häuy, or M and K1 of Phillips.

In the first and third of these instances the crystallographic axes of the combining bodies are parallel. In the second they are perpendicular to each other, the primary form of Rutile being a square prism.

H. J. B.

LVII. *On Peroxide of Manganese containing Silver, from Mexico.* By JOHN TAYLOR, *Esq., F.R.S., Treas. Geol. Soc.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I AM not aware that any ore of manganese has been noticed as containing silver, and I shall be glad to know whether is combination is a new one or not.

Some time since Captain Rule informed me that manganese had been discovered in the Santa Ynez vein in Real del Monte, and that it contained silver enough to be profitably extracted by smelting.

I requested him to send me some specimens, which I have received; they are similar in appearance to the common peroxide of manganese found in this country.

By an assay made of an average sample by Mr. Percival Johnson, it was found to contain silver at the rate of 12 oz. 16 dwts. 16 grs. in the ton.

By a more complete analysis of the mineral the same gentleman found its composition to be as follows:

Peroxide of manganese	30·6
Oxide of Iron	12·5
Silica	21·0
Alumina	17·6
Lime	1·2
Water	16·7
Silver and loss	·4
	100·

Another mineral found in the same vein, which the Mexicans

call Jabon, is steatite intermixed with black particles. The steatite contains no silver, but the black matter is rich in manganese and iron, and contains by Mr. Johnson's assay 185 ounces of fine silver in the ton.

The processes of reduction in the large way in Mexico gave a larger proportion of silver than that indicated by the samples tried here.

I am, Gentlemen, yours, &c.

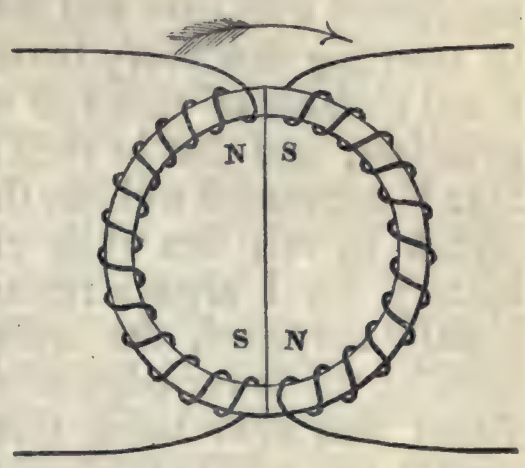
Chatham Place, March 18, 1837. JOHN TAYLOR.

LVIII. *On the Electric Spark and Shock from a permanent Magnet.* *By the* Rev. WM. RITCHIE, *LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution and in University College, London.**

IN mechanical science it is well known and universally admitted, that the *prime* mover can never produce a greater power than that which it possesses; and that *action* and *reaction* are perfectly equal and opposite in their effects: but these great principles are too often lost sight of when we enter on the investigation of the actions of those agents we term *imponderable*. In the case of a wire conducting voltaic electricity, Dr. Faraday has shown that it *communicates* by induction a similar electric state and *loses* a part of its own power. If measurements could be accurately employed there could be no doubt that the quantity *gained* by the one would be exactly equal to the quantity *lost* by the other. In the case of a permanent magnet, or one of hardened steel, there seems still a good deal of obscurity in its mode of action, as if its magnetism *arranged* by induction were *held fast* by that property of the steel called by French writers *la force coërcitive*. That this is *not* the case, and that the electricity in a permanent magnet moves within the limited spaces of the crystalline particles with great facility, may be shown by many examples. If a circle or ring be formed of steel, welded at the junction and rendered very hard, so as to be easily broken with a blow from a hammer, and if it be magnetized by passing one of the poles of a magnet, a north pole for example, round it continually in the same direction as indicated by the arrow, the electricity will be arranged in a peculiar manner, constituting magnetism *without* development of poles. If a covered wire be rolled round it, as in the annexed figure, and the ends covered with the cups of a galvanometer, or so connected that the circuit may be broken at the moment the magnet is broken across

* Communicated by the Author.

by a blow with a hammer, the needle will be deflected, or a spark will appear at the points of the wire, or if connected with the human body a shock will be received. It is obvious, therefore, that the electricity in this circle, at the moment of fracture, is in rapid motion towards a perfectly new state of *stable* equilibrium. From this new state then the electricity may be easily put in motion by an *inducing* or *reacting* cause till it



approach or finally gain the state of *tension* which it had before the ring was broken. Hence from what is called a permanent magnet we may obtain as *powerful* a shock and as *brilliant* a spark as from a soft iron electro-magnet. If in the common magneto-electric machine a continuous wire be coiled round the ends of the permanent magnet, and a simple flat lifter, without a coil, be made to revolve opposite the poles, an exceedingly brilliant spark, and a shock too powerful to be endured, may be easily produced. If the revolving keeper have also a coil, by joining the ends to form a continuous coil with that on the magnet the effect may be much increased.

Instead of the revolving lifter of soft iron, another permanent horseshoe magnet may be employed with equal or perhaps greater advantage. The result of these arrangements may form the subject of another short communication.

LIX. *On the Development and Action of Electricity in Voltaic Combinations.* *By* F. W. Mullins, *M.P., F.S.S., M.R.I., &c.*

DOES the development of a certain force or power of voltaic electricity, whether in the production of quantity or intensity effects, depend upon the employment of equal surfaces of zinc and copper? This question has been frequently answered, by some in the affirmative, by others in the negative, but the prevailing opinion appears to be in favour of equal quantities of the two metals; and in the various forms of batteries in general use we see such arrangements as give equal metallic surfaces, or nearly so. It is therefore because this point is still in dispute that I make my observations pub-

* Communicated by the Author.

lic, in the belief that the results of a series of experiments, conducted with care and frequently repeated, fully justify me in asserting that every battery in present use, which contains equal surfaces of zinc and copper, is constructed on a wrong principle, and that in such batteries an enormous quantity of zinc is consumed without the slightest advantage. I was first struck by this fact while experimenting with the Wollaston battery, for I always remarked that the zinc plates were unequally corroded, the action being much greater within an inch of the lower edge of the plates than higher up. I also found that after using these plates for some time, when of course the original extent of surface was much diminished, the power of the battery was quite as great as at first, assuming that the plates were clean and the battery fresh charged. In consequence of these observations I reduced the size of the zinc plates to a fourth of what they had previously been, and could not perceive the least diminution in effect; so that without any *greater* action in a given time on that part of the zinc which I retained, I was enabled to develop the same power, thereby avoiding the waste caused by the action of the solution on the remaining parts of the larger plates. Having thus satisfied myself that a large quantity of zinc absolutely went for nothing in the common batteries, I entered upon a new series of experiments on the same subject with my sustaining battery, which, in consequence of its power not diminishing, gave me a certainty of still more accurate and satisfactory results than I could have hoped to obtain from a battery whose power was unequal. In order that others who may be prejudiced in favour of equal metallic surfaces may have an opportunity of convincing themselves of the truth of what has been stated, I think it may be well to give them an account of a few of the experiments made with the sustaining battery, which they may perform without any difficulty.

One of the tests of which I availed myself was the magnetic voltameter, which, for the measurement of small quantities of electricity or of that of low tension, is, without doubt, as accurate an instrument as any we know. In these instruments there is of course this defect, that no two of them will, for the same quantity or force of electricity, afford the same indication in degrees, which arises from difference in the size of the needles, length and number of coils, thickness of the wire, &c.; but where the *same* instrument is applied in the comparison of different effects, it is, as I before stated, as perfect as any with which we are acquainted. If then we connect this instrument with one of my small cylinder batteries, the copper in

which is about five inches high, and is surrounded by a zinc cylinder one fourth of the surface of the copper*, the needle will be deflected to an angle of, say 75°: now, if we remove the first cylinder of zinc and substitute another of twice its surface, we shall find no increased deflection, and therefore no increase of quantity. In like manner, if we go on enlarging the zinc surface until at length it equals that of the copper, the indication of the needle will still be the same, and this though we have added a large proportion of the two solutions for the purpose of bringing the entire zinc and copper into contact with the fluids. So far I agree with Mr. Daniell, who also uses small surfaces of zinc; but in prosecuting this experiment it will appear that I differ as much from that gentleman's conclusions, if he says that a thin wire of zinc, or a piece of zinc reduced to the smallest proportions, will produce no diminution of power, as in the former part of the same experiment I differed from Marianini, who asserted that the copper should be at least *eight times* the surface of the zinc in order to produce the maximum effect; for if instead of increasing the zinc surface we now reduce that which was first used, the needle will then diminish its angle and go on retrograding in proportion as the zinc is reduced, thus showing that there is a certain proportion between the zinc and copper surfaces which produces the greatest power. Again, with an electro-magnet of sufficient size, the battery described, when in proper order, will lift two hundred weight; add the larger zincs until you bring the surfaces equal; the lifting power is no greater than at first. Again, take one of my intensity-sustaining batteries, which consists of three zinc and three copper cylinders *one within the other*; apply this instrument to the magnetic voltameter, which will give an angle of 86°; then apply it to Faraday's decomposition voltameter, and the quantity of the gases produced will be found to be exactly the same, whether the small cylinders of zinc be used or larger ones equal in surface to the copper, and the needle will indicate no greater deflexion in the one case than in the other. Fully convinced, therefore, by the results of these and many other experiments too numerous to detail here, that zinc plates or cylinders about one fourth of the surface of the copper, produce fully as great an effect as if the surfaces were equal, I strongly recommend any student of this branch of science, if he still has any doubts as to the correctness of my

* It may be well to observe that in charging this battery I use two fluids: one consisting of 1 part of saturated solution of muriate of ammonia to 4 of water, in contact with the zinc; the other, a saturated solution of sulphate of copper, in contact with the copper; a bladder, or what is better, if properly managed, white silk, being interposed between the metals.

results, to experiment for himself before he goes to the additional expense of large quantities of zinc, and of their necessary consequences, increased expenditure of muriate of ammonia and sulphate of copper, or acids, if he prefer using them. The mode of action of the sulphate of copper in the voltaic circuit is singular, and appears not to have been noticed by any of those gentlemen who have hitherto used it. The opinion seems to be, that in order to keep up the electrical action in regard to quantity as well as tension, it is necessary that the solution should be kept in a state of saturation. This is not the case; the solution does not require the addition of crystals until nearly every particle of copper has been precipitated and the liquid has lost almost all its blue tinge, which fact clearly shows that the effects do not depend upon the quantity of copper in solution, but upon a certain quantity precipitated in a given time, and that so long as that quantity remains to be precipitated, so long is there no diminution of power. In my batteries, which I have often in action for two or three months, I never keep the solution in a state of saturation; and whenever I find that the precipitation has been nearly completed, I can draw off the original charge (of the sulphate) and introduce a fresh supply, without the least interference with the electric action. In cases where crystals are kept in the solution, I have strong grounds for thinking that the action of the liquid on these crystals has a tendency to interfere with the full development of electricity in the solution. It would appear from this peculiar property, if I may so call it, of the sulphate, as well as from the results of many experiments which I hope to detail in a future paper, that a large proportion of the electricity which becomes sensible in this case is produced by the return of the copper to its metallic state, which change obliges it to disengage a certain proportion of the electricity with which it was previously combined in its state of a salt; and further, that the definite proportions of all elements in their various combinations depend upon the proportions of the electric æther with which the material molecules are either accompanied or combined;—that this æther governs the definite proportions of all combinations, no two elements having similar proportions of electricity combining, and those which have *different* proportions uniting;—that as there is but one electric fluid, if it can be so called, *negative* electricity is an improper term, *less positive* being more appropriate;—that electricity is capable of expansion and contraction under certain circumstances;—that in all cases where two or more elements are combined, if the addition of another causes new combinations, separation of one of the original elements, or any other change,

the element which has been added is either more or less positive than either of the original elements, so that, under such circumstances, the material molecules will be instantly subjected to new attractions, the less positive molecule quitting that for which its attraction was not so strong, and uniting with the other for which it has a greater attraction;—that in proportion to the comparative specific gravities of gases, liquids, and solids, so are the quantities of electricity combined with them,—that element of the gases having the *least* specific gravity being the *most* positive, and that having the *greatest* least so*, and in like manner with liquids and solids;—that heat is merely a property of electricity, becoming sensible in chemical decompositions and combinations, by its disengagement in large quantity and the difficulty of restoring the equilibrium; and this it is which causes a platina wire to become red hot when the electric current is sent through a reduced and different conducting medium. Thus, if we revert to the action of the battery in the precipitation of the sulphate of copper, a new attraction being brought into play, more powerful than that of the oxide for the acid, *that* union is dissolved, and the metallic molecules being brought into a state of aggregation, do not in their new state attract the same proportions of electricity; the consequence is that a large quantity, like latent heat in cases of condensation, and, indeed, *identical* with it, is disengaged, and goes to supply the loss of electricity in the circuit.

I believe light, as well as heat, to be a property of electricity, else, how account for its existence in its *purest form in vacuo*, where electricity is the only agent? But I shall refer to these subjects again and at greater length when I have more leisure than I have at present, merely adding that I do not believe my views to be irreconcilable with Mossotti's theory, and

* As an example, I give a list of a few of the metals, in which I institute a comparison between their specific gravities, their atomic numbers, and their electrical states.

	Sp. Gr.	Atom. No.	Pos. Elec.
Potassium .	0·865	40	1
Sodium ...	0·972	24	2
Manganese	6·850	28	12
Zinc	6·861	34	13
Iron	7·788	28	15
Nickel	8·279	26	16
Cobalt	8·538	26	17
Copper ...	8·895	64	23
Silver	10·474	100	24
Platinum ...	20·98	96	27

that I am quite satisfied that though chemical action may be supposed to develop electricity, still electricity itself is the prime mover; electrical and material attractions and repulsions, when brought into play by certain arrangements of elements, inducing and creating all chemical phænomena.

February 7, 1837. F. W. MULLINS.

LX. *Reviews, and Notices respecting New Books.*

The Human Brain, its configuration, structure, development, and physiology; illustrated by references to the nervous system in the lower orders of animals. By SAMUEL SOLLY, Lecturer on Anatomy and Physiology in St. Thomas's Hospital. London, 1836, 12mo.

THIS work of Mr. Solly professes to treat of the development and structure of the human brain as illustrated by a reference to the central portions of the nervous system of the lower animals. We have perused the work with much attention, and no inconsiderable degree of gratification, and are free to confess that the object proposed has been faithfully accomplished. A systematic work of this description has been much wanted as a class-book in our medical schools, where the anatomy of the brain is almost invariably taught as if the organ consisted of isolated fragments of cerebral matter having no communication with each other. Mr. Solly, as a teacher of anatomy at St. Thomas's Hospital, has of late years been in the habit of illustrating his lectures on this subject by continually placing before the student the analogues of many parts of the brain in other animals, and has thrown an interest into this branch of the subject, which as treated before was dry and insipid.

The first part of the work treats of the nervous system of the lower animals, and proceeds to the consideration of that of animals of a higher grade, having more especially in view the law by which masses of 'neurine,' termed ganglia, are concentrated, in proportion to the higher development of the senses of the animals. To illustrate this part of his subject, amongst many other interesting points, the author has adduced the anatomy of the nervous system of the moth, and has shown the progressive development of the organism from the larva to the imago, "and the striking increase in the size, and the greater complexity in the form of the nervous system, when the animal becomes fitted to receive impressions from the objects which surround it, which it does through the medium of *especial organs of sense*."

Mr. Solly has in the elucidation of this part of his subject borrowed largely from the labours of other naturalists, but his extracts from the works of others are faithfully acknowledged.

The anatomy of the human brain forms the next division of his subject, and his method of dissecting the organ accords with that of Reil and Spurzheim. The anatomy is strict and minute, and our author has made us acquainted with some new facts connected with the intricate structure of this complex organism. His description of the fornix as the "inferior longitudinal commissure," differs in some points from that of other authors, and is illustrated

by diagrams taken from preparations now extant, and open to the inspection of any scientific person. The "superior longitudinal commissure" is also new to us. But the most important addition to our knowledge resulting from the labours of Mr. Solly consists in his discovery of some distinct fibres of medullary matter connecting the cerebellum with the anterior columns of the medulla spinalis: this connection is most satisfactorily proved, and the solution it affords to the understanding of many hitherto anomalous facts in pathology renders this a point of high interest. An accurate description of the origin of the motor nerves from, and the termination of the nerves of sensation in, the central organ of the nervous system next follows; and here again some new features are apparent. The last part of the work is dedicated to the physiology of the brain as elucidated by pathological facts; but we forbear to enter upon this subject, as it is one of such immense extent. The work is illustrated by twelve beautiful engravings, and the whole is highly creditable to the author. * * *

LXI. *Proceedings of Learned Societies.*

ZOOLOGICAL SOCIETY.

[Continued from vol. ix. p. 522.]

August 9, 1836.—A SPECIMEN was exhibited of an *Ortyx* which Mr. Gould regarded as hitherto undescribed. At the request of the Chairman he pointed out the distinguishing peculiarities of this new species, which he named and characterized as *Ortyx ocellatus*. This bird differs from *Ortyx Montezuma* in several particulars, but to that species it is most nearly allied.

Mr. Gould also brought before the notice of the Meeting two new species of *Birds* from New South Wales, where they had been collected, and subsequently presented to the Society by Captain Sturt. They are referrible to the genus *Zosterops* of Messrs. Vigors and Horsfield; a group among the *Sylviadæ,* and of which but two species were known at the time those gentlemen instituted the genus. Mr. Gould placed on the table six additional species, a portion of which was from the Society's collection, and the remainder from his own. In the course of his remarks, Mr. Gould adverted to the surprising augmentation of species which has now taken place in nearly every group in ornithology; and characterized the new species mentioned above as *Zosterops albogularis,* Gould, and *Zost. tenuirostris,* Gould. They are the two largest known species of the genus.

Notes by W. C. Williamson, Esq., Curator to the Natural History Society, Manchester, on the appearance of rare Birds in the vicinity of Scarborough, were then read, of which the following is an abstract.

"The prominent position of Scarborough with its projecting headlands separated by deep bays and its high hills covered with wood, render the neighbourhood a favourite retreat for various tribes of birds. Among the spring visitors the *Siskin* may be enumerated,

which appears in April, remaining only a few days apparently on its route to breeding-places farther north. It is never seen at any other period of the year, though considered by authors as a winter visitor. Several examples of the *Hoopoe*, and one specimen of the *Roller*, have been shot in the neighbourhood. The stomach of the latter was filled with the *elytra* and other remains of a species of *Curculio*. Of the *Water Ouzel* or *Dipper* it is stated that, when flying down a stream it drops into the water and dives under any rails laid across from bank to bank, rather than fly over them, rising on the opposite side and pursuing its course. The nest of this bird is occasionally seen so placed under a projecting ledge that a fall of water was constantly rolling over it, thus rendering it secure from any attacks: the birds entering by the sides of the fall.

"The *Redwing* has been seen as late as May; these birds are remarkable for a peculiar cry uttered when disturbed and about to take flight.

"The *Hooded Crow* has been known to breed near Scarborough on two or three occasions. In one instance, a female *Hooded Crow* was observed to pair with a *Carrion Crow* on a large tree at Hackness, where they succeeded in rearing their young. The *Carrion Crow* was shot by the gamekeeper, but the following year the *Hooded Crow* returned with a new mate of the same sable hue as the former one to her old nest. The carrion and young crows were again all shot; the old female by her vigilance escaped all the efforts of the keepers to destroy her, and a third time returned with a fresh mate; she was not however again so successful, but was shot, and is now preserved in the Scarborough Museum. The young birds varied, some resembling the *Hooded* and others the *Carrion Crow* in their plumage.

"The *Great* or *Thick-kneed Plovers* breed on the fallows, and often startle the midnight traveller by their shrill and ominous whistle. This is supposed to be the note so beautifully alluded to by Sir Walter Scott in his poem of The Lady of the Lake,

> 'And in the Plover's shrilly strain
> The signal whistle's heard again.'

for it certainly sounds more like a human note than that of a bird.

"The *Rough-legged Buzzard* breeds occasionally in a precipitous dell near Hackness. A marked female returned the following year with a new mate to her former favourite haunt.

"Three species of the genus *Lestris*, the *Glaucous Gull*, *Little Gull*, *Great Northern Diver*, *Little Auk*, and *Long-tailed Duck* are obtained generally during the prevalence of strong north-easterly winds. Temminck's *Tringa* and the *Olivaceous Gallinule* have been killed near Scarborough. The *Sanderling* visits the shore in May and September. Good sport is sometimes gained at *Woodcock*-shooting in March, when from any cause these birds are prevented continuing their journey northward. In one or two instances a *Woodcock* has been seen there as late as June."

August 23, 1836.—Thomas Bell, Esq., in the Chair.—In consequence

of the lamented decease of the Secretary, E. T. Bennett, Esq., the usual routine of scientific business was suspended.

September 13, 1836.—A communication was read from J. B. Harvey, Esq., of Teignmouth, a Corresponding Member of the Society, on the occurrence of four specimens of the *Velella limbosa* of Lamarck, which were found on the beach at Teignmouth after a continuation of southerly winds and smooth water.

A specimen was forwarded for the Society, and representations of it in four different points of view accompanied the communication.

Mr. Vigors called the attention of the meeting to a *Bird,* presenting a singular form among the *Tinamous,* which he had exhibited at one of the evening meetings in the year 1832, but which, from accidental circumstances, had not been characterized in the Proceedings. The birds of this group, which forms an immediate connecting link between the *Tinamous* and the *Bustards,* were first observed by Mr. Pentland on a high elevation in the Andes, and the specimen before the meeting was brought by that gentleman to this country and presented to the Society. Mr. Vigors described in detail the characters of the genus, to which he assigned the name of *Tinamotis,* and also pointed out the specific characters of the bird, to which he had on a former occasion given the name of *Pentlandii,* in honour of the distinguished traveller who first discovered the group.

Tinamotis.

Rostrum forte, subrectum, *Otidis* rostra persimile; culmine plano.

Alæ mediocres, rotundatæ; *remigibus* primâ et septimâ ferè æqualibus, brevissimis, tertiâ et quartâ longissimis.

Pedes tridactyli; *tarsis* sublongis fortibus; *acrotarsiis* reticulatis squamis inferioribus grandibus; *digitis* longitudine mediocribus, medio cæteris, quæ sunt ferè æquales, longiore, omnibus membranâ utrinque marginatis; *acropodiis* scutellatis, squamis maximis; *unguibus* grandibus, planis, dispansis.

Cauda brevis, subrotundata.

Tinamotis Pentlandii. *Tin. corpore cinereo-brunneo sordidoque fulvo fasciato, capite colloque similiter striatis; crisso femoribusque rufis; mento albescente.*

Plumulæ *capitis colli ventris*que magis albido, *dorsi caudæ*que magis fulvo notatæ; narum notis maculis simulantibus. Longitudo *corporis,* 15; *alæ,* a carpo ad apicem remigis 3tiæ, 10; *rostri* ad frontem, $1\frac{1}{8}$, ad rictum, $1\frac{5}{8}$; tarsi, 2; *digitorum,* unguibus inclusis, medii, $1\frac{5}{8}$, externorum, $1\frac{1}{4}$.

Mr. Vigors took the same opportunity of describing and naming two *Parrots* in the Society's Collection, one of which, now alive in the Menagerie, distinguished by a brilliant purple plumage over the head, nape, and breast, and which came from South America, he characterized under the name of *Psittacus augustus*; the second, of which two specimens had been procured from the late Rev. Lansdown Guilding's collection, received from the Island of St. Vincent, but the precise locality of which was not known, he described by the name of *Psittacus Guildingii.*

Mr. Gould, at the request of the Chairman, exhibited to the

Meeting two tribes of *Birds*, viz. the *Tamatias*, from the warmer parts of America, and the *Coursers*, from the arid regions of Africa and India. Mr. Gould observed, that of the first group, only five species appear to have been known to Linnæus; eleven others had since been added, making sixteen: the Society's collection contained thirteen species. Mr. Gould exhibited a series of drawings in illustration of the group, and characterized one new species under the name of *Tamatia bicincta.*

Mr. Gould stated in conclusion, that this formerly limited group now constitutes a considerable family, or subfamily, whose members appear naturally to form themselves into at least three or four genera: thus divided, the genus *Tamatia*, Cuv. (*Capito*, Vieill.) contains 9 species, that of *Lypornix*, Wagl., 3 species; that of *Monasa*, Vieill., 3 species; and that of *Chelidoptera*, Gould, 1; the latter being a generic title provisionally instituted by Mr. Gould for the *Lypornix tenebrosa*, Wagl., a species which differs in many essential characters from all the other members of the group, possessing as it does a very lengthened wing, and being in every way adapted for powerful flight. He observed, that he had consulted with M. Natterer on the propriety of separating this bird from the other members of the group, in which opinion that eminent naturalist had coincided, and at the same time stated, that it usually resorted to the topmost branches of the trees, whence it sallied forth over the forest in search after its insect food, while, on the other hand, all the other members of the group kept to low thickets and the neighbourhood of the ground. In their general economy they offer a striking resemblance to the *Shrikes* and *Flycatchers*; they are, however, more indolent in their disposition, and sit motionless on a dead branch for hours together, until their attention is drawn to some passing insect, when they sally forth, capture it, and return to the same branch, which they are known to frequent for months together. With the exception of three or four species all the members of this group are confined to the Brazils.

Mr. Gould exhibited six species of the genus *Cursorius*, one of which was described as new by the appellation of *Cursorius rufus*.

This new species of *Cursorius* was from the islands of the Indian Ocean, but from what particular locality Mr. Gould had not been able to ascertain. It differs from *Curs. Asiaticus*, by being smaller in all its proportions, by having the whole of the upper surface of a rich rufous brown, and by not possessing a white band across the rump. In its affinities it is closely allied to both *Curs. Asiaticus* and *Curs. Temminckii.*

Mr. Martin placed on the table two examples of the *Potto* or *Kinkajou* from the Society's Museum, and, at the request of the Chairman, read some notes describing the differences in colour, size, and comparative measurements of parts in the two specimens, of which the following is an abstract.

"The differences which exist in two specimens of the *Kinkajou* in the Society's Museum have led me to introduce them to the attention of the Meeting, as it is not improbable that they may ultimately prove to be distinct species. The *Kinkajou*, however, is so rare

an animal both in the museums and menageries of our country, that we want the means of ascertaining whether or not, like that allied animal the *Coati,* its colour be subject to variations of tint and marking. But independently of the great difference in colour which obtains in the two specimens before the meeting, and on which, taken as a solitary character, we should hesitate to ground a specific distinction, at least until we had compared several specimens, it appears that the ears of the rufous specimen (which was lately presented by George Vaughan, Esq.) are more elongated than those of the other, which died in the Society's Menagerie, where it had lived for many years. It is on this difference, rather than on that of colour, that I have suspected a specific distinction; though I confess my suspicions are strengthened by the latter as a concomitant. A knowledge of the precise localities from which each specimen was obtained would be of great use, but on this point, unfortunately, I have not been able to gain any information.

"In distinguishing between the two species of *Kinkajou,* I consider it best to drop entirely the specific title *caudivolvulus,* (which is applicable to both, and is descriptive rather of a generic than a specific character,) the only mode in fact by which to avoid all possibility of confusion.

"Our first species will stand as *Cercoleptes megalotus.* It is distinguished by the form of the ears, which are elongated, narrow, rounded at the tip, and somewhat flapping; their length is 1 inch 3 lines, their breadth 7 lines.

"Internally they are sparely covered with thinly set soft hairs; externally they are fully clothed with hairs of a pale yellowish white.

"The fur is close, short, thick, and rigid; the general colour is deep reddish yellow, or fulvous, with an obscure band of a darker colour, down the top of the head, the back, and upper surface of the tail, approaching to chestnut. The sides of the body and the insides of the limbs are pale fulvous; the abdomen and throat are nearly as dark as the back, and a stripe of deep chestnut commences about the end of the sternum, and is continued to the inguinal region. The tail is slender, and the hairs of this part are very rigid.

"To our second species we propose to give the name of *Cercoleptes brachyotus.*

"The fur is full, soft, and moderately long; of a universally glossy yellowish gray clouded with brown, especially over the nose, on the top of the head, and down the back; and indeed little less so on the sides of the body and outer surface of the limbs. The abdomen, the insides of the limbs, and the throat are dusky straw colour. The ears are broad, short, and rounded; covered, but somewhat sparingly, on the outside with fur of the same colour as that of the body: their length and breadth are equal, namely, 1 inch.

"The tail is moderately thick, being covered with fur of the same character as that of the body."

Sp. 1. Cercoleptes megalotus. *Cercolept. lætè rufus, strigâ saturatiore, per totam longitudinem capitis, dorsi medii, caudæque*

suprà excurrente; lateribus pallidioribus; abdomine gulâque rufis, strigá castaneá abdominali; auriculis longis, angustis, rotundatis subpendentibus et externè pilis pallidè flavis indutis, caudá gracili; vellere denso brevi, atque rigido.

Sp. 2. Cercoleptes brachyotus. *Cercol. vellere denso, molli, et longiusculo, griseo flavescenti, at brunneo, undato, hoc colore in capite, summoque dorso, saturatiore: abdomine et gulá stramineis auriculis latis, mediocribus, et erectis, pilis rarioribus fuscis externè indutis.*

September 27, 1836.—A communication from Edward Fuller, Esq., of Carleton Hall, near Saxmundham, was read, which stated that his gamekeeper had succeeded last year in rearing two birds from a barn-door *Hen*, having a cross from the *Pheasant*, and a *Pheasant* cock; that the birds partook equally of the two species in their habits, manners, and appearance; and concluded by presenting them to the Society.

The gamekeeper of Edward Fuller, Esq., in a short note which accompanied the birds, stated that he had bred them, and that they were three-quarter-bred *Pheasants*.

The living birds were exhibited at the Meeting, as was also a living hybrid, between the *Pheasant* and *common Fowl*, which was one of several that had been some years in the Menagerie of the Society.

Several specimens of hybrids, from the preserved collection in the Museum of the Society, were placed on the table for exhibition and comparison. These had been bred between the *Pheasant* and *common Fowl*, the *common Pheasant* and the *silver Pheasant*, and the *common Pheasant* with the *gold Pheasant*.

The specimens of the three-quarter-bred *Pheasants* were considered interesting, the opinion of the older physiologists having been that animals bred between parents of two distinct species were unproductive.

Mr. Yarrell stated, that although generally such an opinion prevailed there were still exceptions. The Proceedings of the Society for 1831 exhibited one already recorded in Phil. Mag. and Annals, N.S., vol. xi. p. 138. This communication was received from the Honourable Twiselton Fiennes, who having succeeded in rearing a brood between the *common Duck* and the *Pintail*, found in the following season these hybrids were productive. Other instances are also on record which were adverted to. Mr. Yarrell stated, that he had had opportunities of examining the bodies of hybrids, both of *Gallinaceous Birds* and *Ducks*, and found that the sexual organs of the males were of large size, those of the females deficient in size, and not without some appearance of imperfection. The crosses produced by the breeders of *Canaries* were mentioned, and the objects of obtaining them explained. Mr. Yarrell expressed his belief that the attempt to breed from a hybrid was most likely to be successful when a male hybrid was put to a female of a true species.

Mr. Vigors said this was the first instance that had come to his knowledge of a female hybrid being productive, and he had hitherto

considered that they were not so: he expressed his desire to see the female hybrid that had produced the three-quarter *Pheasants* then in the room, and hoped that the opportunities which the Menagerie of the Society afforded of obtaining additional evidence on this interesting subject would not be lost sight of.

The Chairman (Mr. Owen) stated, that it was the opinion of John Hunter that hybrids were not productive except in cases where the generative organs were in a state of perfection, which might be regarded as unnatural in hybrids, as in the rare cases recorded of fertile *Mules*, between the *Horse* and *Ass*. Constant fertility in the hybrid proved, in the opinion of Hunter, that the parents were varieties of the same species, not distinct species. But the Chairman stated, that the experiments recorded by Hunter in the 'Animal Œconomy' relative to the fecundity of the hybrids from the *Dog* and *Wolf* and *Dog* and *Jackal* were incomplete, from the circumstances of the hybrids having always bred from a perfect species and not having propagated the intermediate variety *inter se*. He trusted that in a short time this test would be applied in experiments now in progress at the Society's Menagerie, and thus an additional element be gained towards the solution of this interesting question.

A small collection of *Birds* from Swan River, presented to the Society by Lieut. Breton and Capt. Brete, were on the table. Mr. Gould, at the request of the Chairman, observed upon the collection generally, and selected two species which he considered as undescribed, a *Gallinule* and a species of *Duck*, the latter strictly referrible to the genus *Oxyura* of L. Bonaparte, Prince of Musignano, (genus *Undina* of Gould). Mr. Gould named the *Gallinule, Gallinula ventralis*, and the *Duck, Oxyura Australis*, this being the only instance he had seen of this limited group from Australia. Of this species the collection contained both male and female, the latter of which, in the general distribution of its markings and colouring, bore so close a resemblance to the *Hydrobates* of Temminck that the bill alone presented the obvious distinction.

Oct. 11, 1836.—A series of *Mammalia* selected from the collection of the Society was exhibited. Mr. Gray made some remarks upon them illustrative of the value which he conceived was to be placed on the characters used by M. Cuvier to separate the plantigrade from the digitigrade *Carnivora*, and he concluded by stating that he did not regard the nakedness of the sole as a good character to separate the genera into larger or smaller groups, though from its permanence in all ages and the state of the species, it furnished excellent characters to distinguish species, to separate them into sections, and often to characterize the genera of carnivorous animals; and in proof of the latter, he referred to the excellent character which it furnished to distinguish the species of the genera *Herpestes*, *Mephites*, and *Lutra*. He further observed, that in many instances the extent of the nakedness of the soles appears to depend upon the temperature of the country that the animal inhabited, and mentioned that several of the animals living in countries covered with snow, which apply the

whole of the soles of their feet to the ground, have this part entirely covered with hair, as the *Wolverine*, the *Panda*, the *Seals*, and the *Polar Bear*; but that this was not universally the case, for the *Benturing*, which inhabited the same country as the *Panda*, has the soles bald and papillary. He further observed, that the nakedness of the soles did not appear to be permanent even in the specimens of the same species in the *Squirrel* and other *Glirine* animals; for he had observed that the specimens of the *grey Squirrels*, in the Northern part of the United States, had this part covered with hair, whilst those of the Southern parts, had the soles entirely bald; and he also observed, that the various species of the *Spermophile* differed greatly amongst themselves in the extent of the nakedness of this part.

Mr. Gray then proceeded to make some remarks on the alteration in the situation of the teeth, and on the change which takes place in the form of the carnivorous tooth, in the milk and permanent teeth of the *Carnivora*; and stated, that the milk carnivorous tooth of the *Cat*, *Dog*, *Vison*, *Skunk*, *Viverra*, and indeed of all the genera which he had been able to examine, had a small central internal lobe, whilst the same tooth in the permanent set always had a large anterior lobe; he also stated, that he had observed that the tubercular grinders of the *Mustelæ* often vary considerably in size in the various specimens of the same species, showing that implicit reliance cannot be placed in the size of these teeth as a specific character, which several persons have been inclined to do, as it is well known that the size of such teeth does not depend upon the age of the animal, as they never alter their size after they are once completely developed. Mr. Gray then proceeded to point out the characters by which the new species exhibited were distinguished: two were said to have formed part of the collection of the late Sir Stamford Raffles, and were therefore supposed to have come from Sumatra; one of them was a new species of *Paradoxurus*, called *P. leucomystax* from its strong white whiskers, and the other Mr. Gray regarded as the type of a new genus which he called *Cynogale*, which appeared to be intermediate between *Paradoxurus* and *Ictides*, by differing from both in the length of the face, the compressed form of the false canines, and the small size and triangular form of the carnivorous grinder. Mr. Gray proposed to call it *Cynogale Bennettii*, after his late friend, who, he believed, intended to have described this animal if he had lived. Then followed the description of two *Foxes*, (*C. Magellanicus* and *C. griseus*), which formed part of the collection made by Capt. P. P. King, during his survey of the coast of South America, and a *Squirrel* (*Sciurus Douglasii*), and three *Hares*, (*Lepus longicaudatus*, *L. Californica*, and *L. Douglasii*), discovered by the late Mr. Douglas in North America. Then the description of three new species of *flying Squirrels* from various parts of continental India, viz. *Pteromys Melanotis*, *P. albiventer*, and *P. Leachii*; the latter, presented by Mr. Mellish to the Society, is peculiar for being coloured exactly like the American *Sciuroptera*, but is at once distinguished from them by the length and cylindrical form

of its tail; and an *Herpestes* from the Indian Islands, like the black *Herpestes* of the Cape, but differing from it in colour and in the shortness of the tail, therefore called *H. brachyurus*. Mr. Gray then proceeded to point out the character, taken from the form of the soles of the hind feet, by which the *Skunks* could be divided into three sections or subgenera, and showed the character in the four species in the collection of the Society, and referred to some other species belonging to these sections which were in the collection of the British Museum, where also he stated other specimens of several of the species, as the *Dog, flying Squirrel,* and *Herpestes,* now described, were to be found.

Mr. Gould exhibited several specimens and drawings of *Birds* allied to the well-known *Wren* of Europe; and, at the request of the Chairman, proceeded to comment upon, and characterize the undescribed species as *Troglodytes Magellanicus, Troglod. leucogastra,* and *Thryothorus guttatus,* the latter two species from Mexico.

Mr. Gould also proposed a new genus in the group of *Wrens,* under the name of *Scytalopus,* and which he characterized as follows:

Genus Scytalopus.

Rostrum capite brevius, compressum, obtusum leviter recurvum.

Nares basales, membranâ tectæ.

Alæ concavæ, breves, rotundatæ, remige primâ abbreviatâ, tertiâ, quartâ, quintâ et sextâ æqualibus.

Cauda brevis, rotundata, (pennis externis brevissimis,) laxâ.

Tarsi elongati, atque robusti, antrorsùm scutellis tecti; posteriùs fasciis angustis cincti, squamis serpentum abdominalibus, haud dissimilibus; halluce elongato et robusto; ungue elongato; digitum anteriorum, medio elongato et gracili.

Hoc genus ad illud in quo *Troglodytes* veræ amplectuntur maximam affinitatem demonstrat.

Scytalopus fuscus. *Scy. corpore toto fuliginoso-nigro; capitis plumis nonnunquam argentato-griseis; rostro nigro; pedibus brunneis.*

Long. tot., $2\frac{3}{4}$ unc.; *rostri*, $\frac{1}{2}$; *alæ*, $1\frac{7}{8}$; *caudæ*, $1\frac{1}{4}$; *tarsi*, $\frac{7}{8}$.

Hab. in Fretu Magellanico, Chili, &c.

Scytalopus albogularis. *Scy. capite cæruleo-nigro; corpore superiore ferrugineo-brunneo, lineâ transversali nigrâ; caudâ pallidè rufo-brunneâ; gulâ, pectore, abdomineque intermedio albis, lateribus et crisso pallido ferrugineis lineâ transversali nigrâ; mandibulâ superiore nigrâ brunneâ; pedibus brunneis.*

Long. tot., $3\frac{3}{4}$ unc.; *rostri*, $\frac{5}{8}$; *alæ*, $1\frac{3}{4}$; *caudæ*, $1\frac{1}{2}$; *tarsi*, $\frac{3}{4}$.

Hab. in Brasiliâ.

Oct. 25, 1836.—Two skulls of the *Orang-Utan* of Borneo, and a skin, including the *cranium,* of an immature *Orang-Utan* of Sumatra, were exhibited. They were transmitted to England by Dr. W. Montgomerie of Singapore, with a statement that the young Sumatran *Orang* had died in that gentleman's possession soon after having acquired additional grinders.

Mr. Owen availed himself of the occasion to make the following observations on each of the above specimens.

He stated that the skin of the young Sumatran *Orang* agreed in the rufous colour, texture, disposition, and direction of the hair, with the adult female Sumatran *Orang*, presented to the Zoological Society by Sir Stamford Raffles; like that specimen also, it had no nail on the *hallux* or thumb of the hinder hands*. The posterior *molares* on each side of each jaw correspond to the first permanent *molares* of the adult; the rest of the teeth consisted of the 8 deciduous *bicuspides*, the 4 small deciduous *canini*, and the 8 deciduous *incisores*. This state of the dentition was similar to that of the human child at the 7th year; but it would be unsafe to infer from this circumstance that the age of the *Orang* corresponded: it being more probable, from the characteristic duration of the immature state in the human species, that the shedding of the teeth takes place at a later period than in the *Orang*.

Of the two *crania* of the Bornean *Orangs*, one differed materially from the other in size and in the development of the cranial ridges. The larger specimen before the Society, closely resembled the *cranium* of the Bornean *Pongo* or adult *Orang* in the Museum of the College of Surgeons, and differed, in precisely the same respects as that specimen, from the *cranium* of the *Pongo* (supposed to be Sumatran) in the possession of Mr. Cross, described and figured in the 1st volume of the Society's Transactions, (p. 380. Pl. 53, and noticed in our report of Mr. Owen's paper, in Lond. and Edinb. Phil. Mag., vol. vi. p. 457), which induced Mr. Owen to entertain more strongly his original suspicion, that that *cranium* belonged to an Orang specifically distinct from the great Bornean species (*Simia Wurmbii*, Fischer). With respect to the differences alluded to, he stated that the *cranium* of the great Bornean *Orang* was characterized by the more oblique plane of the orbits, and consequently the straightness of the contour of the skull between the forehead or *glabella* and the incisor teeth; the external boundaries of the orbit were broad and had a rough irregular surface, probably in consequence of the development of the callous protuberances which characterize the sides of the face in the adult males of this species. The *symphysis* of the lower jaw was also proportionally deeper than in the (supposed) Sumatran *Pongo*. The *cranium* of that animal in the possession of Mr. Cross, Mr. Owen regarded as being that of a male individual from its size and from the development of the cranial ridges.

The sexual peculiarities observable in the *cranium* of both the Bornean and Sumatran *Pongos* are well marked, and are exemplified, first in a difference of relative size, that of the female being about $\frac{1}{6}$th smaller; secondly, in a much smaller development of the cranial ridges; and thirdly, in the *symphysis menti* being of less depth, the *cranium* of the female approaching in these respects, according to the usual law of sexual development, towards the characters of the immature animal. The smaller of the *crania* of the two Bornean Orangs, Mr. Owen regarded as indicative of a species of *Simia*, Erxl.,

[* See Mr. Brayley's notes on this deficiency in the Orangs, Lond. and Edinb. Phil. Mag., vol. vii. p. 72.]

equally distinct from the great *Pongo* of Borneo (*Simia Wurmbii*, Fischer, Synopsis Mammalium, p. 32, No. 43), and from the *Orang* of Sumatra (*Simia Abelii*, Fischer, *ibid.* p. 10, No. 2*); and whilst regretting that his conclusion as to the specific distinction of the smaller *Orang*, (which, *cæteris paribus*, must be at least one third less than either of the two preceding *Orangs*) necessarily reposed on a comparison of the *cranium* alone, he at the same time observed that, as the *cranium* in question was in every respect entire, and with the series of teeth complete, it served to establish that deduction on the sound basis of dental and osteological characters.

Mr. Owen therefore proposed to designate the lesser *Orang* of Borneo, *Simia Morio*, and proceeded to describe the *cranium* as follows:

"The size and form of the *cranium* of the *Simia Morio* at first suggests the idea of its being an intermediate stage of growth between the young and adult *Simia Satyrus*, or *Pongo*; but this is disproved by comparison of the teeth of *S. Morio*, with the permanent teeth in the adult *Pongo*, and with the deciduous ones in the young *Simia Satyrus*, as well as with the germs of the permanent teeth concealed in the jaws of the latter. For while the teeth of *S. Morio* are much larger than the deciduous teeth of the young *S. Satyrus*, they have different relative sizes one to another from those which are observed in the permanent teeth of the full-grown: the *molares* and *bicuspides* of the *S. Morio* being smaller, the *canini* much smaller, while the upper *incisores* have nearly, and the lower *incisores* fully, the same dimensions as those of the great *Pongo*.

"The teeth in the jaws of a quadrumanous *cranium* may be known to belong to the permanent series, by the absence of the *foramina*, which, in an immature *cranium*, are situated behind the deciduous teeth, and which lead to the cavities containing the crowns of the permanent teeth. This character is very conspicuous on comparing the *cranium* of *Simia Morio* with that of a young *Simia Satyrus*, in which the deciduous series are present, together with the first permanent *molares*. The deciduous teeth in the young *Orang*, besides their smaller size, are more or less protruded from their sockets, and thrust apart from one another by the *vis à tergo* of their huge successors, while the teeth of *S. Morio* are lodged firmly in the jaws; and, with the exception of the characteristic interval between the canines and incisors, are compactly arranged in close contiguity with each other.

"I have re-examined with much interest several *crania* of immature *Orangs*, in order to ascertain if any of these might be the young of the species in question; but they have all presented the crowns of the permanent *molares* of too large a size,—of a size which shows that the great *Pongo*, either of *Wurmb* or *Abel*, represents their adult state†. And these immature *crania* also indicate the condition to

† The permanent teeth in the Bornean and Sumatran *Pongos* so closely correspond in size and shape that I am unable to refer the *crania* of the immature Orangs which I have hitherto examined to either species exclusively from comparison of the crowns of the concealed permanent teeth;

which they are destined to attain by the size of the orbits, which exceeds that of the orbits of the *S. Morio*, the eye having, like the brain, already in the young *Pongos* acquired its full size.

"That the *cranium* of the *Simia Morio* here described, belonged to an adult is proved by the small interval between the temporal ridges at the crown of the skull, corresponding to the extensive surface of origin of the *crotophyte muscles*; and by the obliteration of the intermaxillary sutures: that it belonged also to an aged individual is highly probable from the extent to which the teeth are worn down, and from the obliteration, notwithstanding the absence of interparietal and lambdoidal crests, of the sagittal and lambdoidal sutures.

"The cerebral portion of the skull of *Simia Morio* equals in size that of the *Pongo*, and indicates the possession of a brain at least as fully developed as in that species, while the maxillary portion is proportionally smaller; so that, as the *cranium* rises above the orbits, and is, like that of the *Pongo*, more convex on the coronal aspect than in the *Chimpanzee*, and wants the prominent supraciliary ridge which characterizes the African *Orang*, it presents in the *Simia Morio* altogether a more anthropoid character.

"There are, however, the rudiments of the ridges which so remarkably characterize the *cranium* of the mature *Pongo*. Those which commence at the external angle of the frontal bone pass backwards, upwards, and slightly converge, but do not meet; they gradually diminish in breadth, and, after passing the coronal suture, subside to the level of the skull; they are then only traceable by a rough line, which leading parallel to the sagittal suture, and gradually bending outwards, rises again to be continued into the lambdoidal ridges; thus circumscribing the origins of the temporal muscles. The lambdoidal and mastoid ridges are broader and more developed than in the *Chimpanzee*, but inferior in both respects to those of the *Pongo*. The inial region of the *occiput* is almost smooth, and is convex, without the mesial ridge, and strong muscular impressions observable in the *Pongo*, where a preponderating weight in front calls for the insertion of powerful muscles behind to counterbalance it."

The temporal bones join the frontal in *Simia Morio* as in the *Troglodytes niger*; but this structure occasionally is present on one or both sides of the skull in *Simia Satyrus*.

The *additamentum suturæ lambdoidalis* is present on both sides

in speaking of the immature specimens of the great *Pongo*, I therefore use the term *Simia Satyrus*; in comparing the *Simia Morio* with the *adult Pongo*, I would be understood as always referring to the Bornean species, with cheek-callosities, or the *Simia Wurmbii* of Fischer. If the specific differences of *Simia Wurmbii* and *Simia Abelii* be admitted the term *Simia Satyrus* must merge into a synonym, as having been applied indiscriminately to the young of both these large *Orangs*. In each case, the generic term *Simia* is applied in the restricted sense in which it is used by Erxleben in his 'Systema Regni Animalis,' 8vo, 1777, and with which the term *Pithecus*, substituted by Geoffroy for the genus of *Orangs*, is synonymous.

in the *S. Morio*, and the beginning of the lambdoidal suture may be faintly traced, but the remainder is obliterated.

Directing our attention to the base of the skull of *S. Morio* we observe the occipital *foramen* to be less posteriorly situated than in the *Pongo*, but more so than in the *Chimpanzee*. The plane of the *foramen* is also less oblique than in the *Pongo*. The occipital condyles are as far apart anteriorly as in the *Chimpanzee*. The anterior condyloid *foramina* are double on each side as in the *Pongo*: the carotid and jugular *foramina* open within the same depression; they are relatively further apart in the *Chimpanzee*: the petrous portion of the temporal bone, as in the *Pongo*, is relatively smaller than in the *Chimpanzee*, and the articular cavity, or surface for the lower jaw, forms a larger proportion of the base of the skull.

The other characters of the *basis cranii* correspond with those of the *Pongo*; and the smaller size of the *meatus auditorius externus* is probably associated in both species with a smaller auricle, as compared with the *Chimpanzee*.

On the bony palate the relative position of the *foramen incisivum* corresponds with the development of the incisive teeth, showing the intermaxillary bones to be of larger size in the *S. Morio* than in the *Chimpanzee*: the situation of the sutures joining these bones to the maxillaries is indicated by vascular grooves, but otherwise obliterated; while in the *cranium* of a young *Pongo* of nearly the same size as that of the *Simia Morio*, the intermaxillary sutures still remain, corresponding to the non-development of the permanent laniaries. It will be interesting to determine at what period these sutures are obliterated in the more anthropoid *Simia Morio*.

The *os nasi* is a single narrow long triangular bone, slightly dilated at its upper end or apex, with the basal margin entire, presenting no indications of original separation into two parts, as has been observed in skulls of the *Chimpanzee*.

In the contraction of the interorbital space, and the general form of the orbit and its boundaries, the *Simia Morio* resembles the *Simia Satyrus*, but the orbital cavity, as before observed, is smaller. In the plane of the orbit and straight contour of the upper jaw, the *Simia Morio* resembles the Bornean species of *Pongo* or *Simia Wurmbii*, rather than the *Simia Abelii* or Sumatran *Pongo*.

The orbital process of the *os malæ* is perforated in the *S. Morio* as in the *Pongo*, by several large *foramina*. There is one principal and two very small infraorbital *foramina* on either side; the upper maxillary bones are relatively smaller, as compared with the other bones of the face, and especially the intermaxillaries, than in the *Pongo*; a structure which coincides with the smaller proportional development of the canine teeth. The nasal aperture has the same form as in the adult *Simia Wurmbii*, being more elongated than in the immature *Orang*.

The main and characteristic difference then between the *Simia Morio* and the *Pongo*, whether of Borneo or Sumatra, obtains in the size of the laniary or canine teeth, to the smaller development of which in the *S. Morio*, almost all the other differences in the *cranium*

are subordinate or consequent. The laniary teeth, it may be observed, have little relation to the kind of food habitual to the *Orangs*; had they been so related they would have been accompanied with a structure of the glenoid cavity fitting them, as in the true *Carnivora*, to retain a living prey in their gripe, till its life was extinguished or resistance effectually quelled. But the flattened surfaces on which the condyles of the lower jaw rotate are in subserviency to the flattened tuberculate molars, showing the mastication of vegetable substances to be the habitual business of the jaws, and the application of the laniaries to be occasional, and probably defensive in most cases. We perceive the utility of formidable canine teeth to the *Orangs*, whose stature makes them conspicuous and of easy detection to a carnivorous enemy; such weapons, in connexion with the general muscular strength of the *Pongos*, enable them to offer a successful defence against the *Leopard*, and may render them formidable opponents even to the *Tiger*; but in the smaller species, which we have been describing, to which concealment would be easier, the canines are of relatively smaller size, and those of the lower jaw are so placed as to be worn down by the lateral incisors of the upper jaw; they were reduced in the specimen described, to the level of the other teeth; and the points of the upper canines were also much worn. The size, forms, and proportions of the teeth which relate more immediately to the food of the *Orangs*, viz. the molars and incisors, show indisputably that the *Simia Morio* derives its sustenance from the same kind of food as the larger *Orangs*. The singular thickness or antero-posterior diameter of the incisors, which are worn down to a flattened surface, like molar teeth, show that they are put to rough work; and it is probable that their common use is to tear and scrape away the tough fibrous outer covering of the cocoa-nut, and, perhaps, to gnaw through the denser shell.

With respect to minor differences not noticed in the description, these may be deduced from the subjoined table of comparative admeasurements.

Table of Admeasurements.

	Simia Morio, adult.		*Smia Wurmbii*, adult male.	
	inch.	lin.	inch.	lin.
Length of the skull from the *vertex* to the base of the occipital condyle....................	3	7	4	6
Length of the skull from the posterior plane of the *occiput* to the margin of the incisors	7	10	10	6
Length of the skull from the posterior plane of the *occiput* to the fronto-nasal suture	4	4	5	3
Length of the skull from the fronto-nasal suture to the margin of the incisors..............	4	1½	5	7
Greatest lateral diameter of the skull (at the post-auditory ridges)........................	4	8	5	4
Smallest lateral diameter of the skull (behind the orbits)..................................	2	4	2	9

	Simia Morio adult. inch.	lin.	*Simia Wurmbii*, adult male. inch.	lin.
Distance between temporal ridges	0	7	0	0
Diameter of the skull at the *zygomata*	5	1	6	9
Length of the zygomatic *fossa*	1	9	2	6
Diameter of skull taken between the outsides of the orbits	3	6	4	6
Interorbital space	0	4	0	7
Transverse diameter of orbital cavity	1	3	1	6
Vertical diameter of orbital cavity	1	6	1	7
Vertical diameter of nasal aperture	1	1	1	6
Transverse diameter of nasal aperture	0	9	1	0
Interspace between infraorbital *foramina*	1	7	2	0
Distance between the inferior margin of the nasal bone and the inferior margin of the intermaxillary bone	2	5	3	3
From the anterior margin of the occipital *foramen* to the posterior margin of the bony palate	2	3	2	10
Length of the bony palate along the mesial suture.	3	1½	4	0
From the anterior margin of the intermaxillary bones to the anterior palatal *foramina*	0	10	1	3
Breadth of the crown of the first incisor, upper jaw.	0	6	0	7
Breadth of the crown of the second incisor, upper jaw	0	3½	0	4
Breadth of the four incisors, *in situ*, upper jaw	1	6	1	9
Longitudinal extent of grinding surface of the *molares, bicuspides* included, of one side, upper jaw	2	2	2	5
Length of the enamelled crown of the canine tooth, upper jaw	0	6½	1	0
Breadth of ditto	0	5	0	9
Length of the lower jaw from the condyle to the anterior surface of the sockets of the incisors.	5	7	7	4
Length of the *ramus* of the lower jaw	3	4	4	7½
Greatest breadth of ditto	2	0	3	1
Interspace between the mental *foramina*	1	8	2	1

Mr. H. E. Strickland read a list of *Birds* noticed or obtained by him in Asia Minor, in the winter of 1835 and spring of 1836.

He stated that the winter of last year was one of unusual severity in all parts of Europe. At Smyrna, where he resided from November to February, the weather, which had been mild in the early part of December, underwent a sudden change about Christmas-day. A north wind and violent storms of snow brought vast flocks of northern *Birds* to take shelter in Smyrna Bay. A frost of more than three weeks followed, a circumstance almost without parallel at Smyrna, which is situated close to the sea and in the low latitude of 38½°. This statement will explain the occurrence in the following list, of many *Birds* whose usual abode is in high northern latitudes.

In the month of February he visited Constantinople, and returned overland to Smyrna, which he reached at the end of April. A great change had now taken place in the ornithology of that neighbourhood. The spring was now at its height, and numerous summer birds had arrived, of a more exotic race than those which had been observed during the winter. Mr. Strickland was now, however, compelled to return to Europe; but the few days which passed before he left Smyrna, served to give him a taste of the rich ornithological harvest which might be reaped by a summer's residence in Asia Minor.

The list, which appears in No. xlvi. of the Society's Proceedings, comprehends 129 species, of which specimens of 73 species had been obtained by Mr. Strickland, and were exhibited, each being distinguished by an asterisk in the Catalogue. The following are extracts:

*32. *Curruca melanocephala,* Bechst. This delicate little bird, which is only found in the most southern parts of Europe, remains through the winter in the neighbourhood of Smyrna. It is a retired solitary bird, frequenting sheltered ravines thickly beset with various evergreen shrubs.

*34. *Sylvia brevirostris,* mihi. Also killed in November near Smyrna. This species, which I believe to be new, may be thus characterized:

SYLVIA BREVIROSTRIS. *Sylv. corpore suprà olivaceo brunneo, subtùs albido; pedibus nigris.*

Plumage closely resembling that of *S. Trochilus.* Above brown with a tinge of olive. A pale yellow streak over the eye. Throat and breast pale fulvous with a slight tinge of yellow; belly whitish. Inner wing-coverts of a pale yellow. *Remiges*: the 4th and 5th longest and equal: the 2nd equal to the 8th. Beak dusky; legs black.

Long. tot. poll. $4\frac{3}{4}$; *rostri,* $\frac{1}{4}$; *caudæ,* $2\frac{1}{8}$; *alæ,* $2\frac{2}{5}$; *tarsi,* $\frac{3}{4}$.

Differs from *S. rufa* in its greater size, and from *S. Trochilus* in the shortness of the beak, and the dark colour of the legs.

Habitat prope Smyrnam. Hyeme occisa.

*56. *Emberiza cinerea,* mihi. This new species is thus characterized:

EMBERIZA CINEREA. *Emb. capite viridi-flavescente; corpore suprà cinerascenti, subtùs albo.*

Male. Crown of the head greenish yellow, becoming cinereous at the nape. Back cinereo-fuscous with an obscure streak of brown in the middle of each feather. Rump cinereous; tail dark brown; the two lateral pairs of feathers white on the inner webs for near half their length towards the extremities.

Wings dark brown, the coverts and quills margined with whitish, the scapulars with fulvous. Chin and throat yellow, becoming greenish on the cheeks.

Breast cinereous; abdomen white, sides cinereous.

Bill dusky; legs flesh-coloured.

Long. tot. poll. 6; *rostri,* $\frac{2}{5}$; *alæ,* $3\frac{1}{2}$; *caudæ,* $2\frac{3}{4}$; *tarsi,* $\frac{3}{4}$.

The beak of this species most nearly resembles that of *Emberiza Cia.*

Habitat in collibus juxta Smyrnam. Mense Aprili occisa.

68. *Corvus Monedula,* Linn. Common near Smyrna.

Obs. The *common Rook* was not noticed, and I do not believe that it exists in the country.

*70. *Garrulus melanocephalus,* Bonelli. This bird was first described by M. Gené in the Memoirs of the Academy of Turin, vol. xxxvii. p. 298, Pl. I., from specimens in the Turin Museum, received from Lebanon. It is common in the vicinity of Smyrna, and its note and habits are identical with those of the European *Jay,* whose place it supplies.

79. *Phasianus colchicus,* Linn. Common near Constantinople on both sides of the Bosphorus. It has probably migrated thither spontaneously from Colchis, its native country.

*86. *Columba cambayensis,* Lath. This bird inhabits the Turkish burial-grounds at Smyrna and Constantinople, which are dense forests of cypress trees. It is strictly protected by the Turks, and it was with some difficulty that I obtained a specimen. It was, perhaps, originally introduced by man, but now seems completely naturalized.

87. *Otis tarda,* Linn. Frequents the plains south of Smyrna. It is called *wild Turkey* by the European residents.

*88. *Otis tetrax,* Linn. Abundant during the winter in the poultry shops at Smyrna.

*94. *Ciconia alba,* Bellon. Very abundant in Turkey during summer. It swarms in every village, and is protected with the same strictness by the Turks as by the Dutch. It is said to have quite deserted Greece, since the expulsion of its Mahometan protectors.

*111. *Podicepsc ristatus,* Lath. The young of this bird isa bundant in the harbour at Constantinople, where, in common with all other waterfowl, it is strictly protected.

*112. *Puffinus Anglorum,* Ray. Flocks of this bird are constantly seen flying up and down the Bosphorus. They are rarely seen to alight, and from their unceasing restlessness, the Franks of Pera have given them the name of *âmes damnées.* I am not aware that this bird has before been noticed in the southern parts of Europe.

Of *Vultur,* Illig., and *Aquila,* Briss., two or three species frequent the neighbourhood of Smyrna, but all Mr. Strickland's endeavours to procure specimens of these wary birds were unavailing.

Mr. Strickland also exhibited the skin of a variety of the *common Fox, Canis Vulpes,* Linn., which occurs near Smyrna: together with a specimen of the *Lepus hybridus,* Pall., from the South of Russia purchased of a furrier at Rome.

Also a specimen of an *Argonauta,* Linn., which was brought to him in Cephalonia with the animal alive in it. Mr. Strickland stated that he kept it for some hours alive, and when dead it fell out of the shell with its own weight; proving that there is no muscular connexion between the animal and the shell. In this instance the shell did not contain any *ova.*

Mr. Ogilby called the attention of the Society to two *Antelopes* at present living in the Gardens, which he regarded as the *Koba* and *Kob* of Buffon. He expressed his pleasure at having it in his power to identify two animals originally described imperfectly, and of which the zoological characters have been hitherto almost unknown; ob-

serving that the re-discovery of an old species was at all times more gratifying to him, and, he considered, more beneficial to the science of zoology, than the original description of twenty that were new; because, whilst it equally added an authentic species to the substantive amount of our knowledge, it had the further merit of dispelling the many doubts and surmises which unavoidably obscured the subject. Mr. Ogilby entered at some length into the identification of these two interesting species, referring to the scanty materials afforded by the original descriptions of Buffon and Daubenton, and pointing out the various other *Ruminants* with which subsequent naturalists had confounded them; at the same time reserving his more detailed demonstration of this subject, and his descriptions of the animals themselves, for the monograph which he has been long preparing for the Transactions of the Society. Among other errors, he pointed out that the *Koba* of Pennant (*A. Senegalensis*) was the *Caama;* and that the *Korrigum* of Denham and Clapperton's Travels, identified with *A. Senegalensis* by Mr. Children and Colonel Smith, was a very distinct animal from the *Koba*, and even belonged to a different natural genus. It has horns in the female sex and lachrymal sinuses, both of which characters are absent in the *Koba*: he therefore proposed to distinguish the Bornou animal by the specific name of *A. Korrigum.* The same observation applies to the two species which Colonel H. Smith has described under the names of *A. Adenota* and *A. Forfex*, and which he identified with the *Kob* and *Gambian Antelope* respectively; both these animals had lachrymal sinuses, whereas, both Buffon and the more accurate Daubenton, expressly declare that the *Kob* is without this character. The animals in the Gardens, however, corresponded in all respects with the original descriptions; their comparative size, their colour, their habitat, their zoological characters, as far as they were reported, and, in the case of the *Koba*, even the name, were identical; and it therefore gave him peculiar satisfaction to be able to congratulate the Society on the possession of two of the rarest and most interesting *Antelopes* ever brought together. He observed, in conclusion, that the female of the *Kob* had been observed by him six or eight months ago in the Surrey Zoological Gardens, but that he had only recognised its identity with Buffon's animal on the arrival of the fine male specimen at present belonging to the Society.

Mr. Ogilby afterwards exhibited the skin of a *Fox* from the Himalayan mountains, which he has described in the Zoological Part of Mr. Royle's "*Flora Himalaica*," under the name of *Canis Himalaicus*. This animal, of which Mr. Ogilby stated that he had examined three skins, two belonging to the Zoological Society, and one procured by Mr. Royle at Mussooree, (the two former in their summer, the latter in its winter dress,) appears to be rare in Nepaul, since Mr. Hodgson has never been able to procure a specimen, but contents himself with indicating its existence (see Catalogue of Mammalia of Nepaul); it is not uncommon, however, in the Doon, in Kumaon, and the more western and elevated parts of the Mountains, where it is called the *hill Fox* by the Europeans, and greatly admired for the beauty of its form, and the brilliancy and variety of its colours. The whole length

to the origin of the tail is 2 feet 6 inches; that of the tail, 1 foot 6 inches; that of the ears, 4 inches; and the height may be about 1 foot 4 or 5 inches. The animal agrees with the common European and American *Foxes*, (*C. Vulpes* and *C. fulvus*,) in the black marks on the backs of the ears, and in front of the hind and fore legs. The coat consists of long close rich fur, as fine as that of any of the American varieties, and of infinitely more brilliant and varied colours. It consists of two sorts of hair, an interior of a very fine cottony texture, and an external of a long silky nature, but perfectly pliant, and, like the fur of the *Sable*, lying almost equally smooth in any direction. The inner fur is of a smoky blue or brown colour along the back, as is likewise the basal half of the outer silky hair, which, up to this point, is of the same soft cottony texture as the interior fur; it then assumes its harsher silky character, is marked with a broad whitish yellow ring, and terminated by a long point of a deep bay colour. Hence, along the whole upper surface of the head, neck, and back, the uniform colour is unmixed deep and brilliant red. On the sides of the neck, on the throat, ribs and flanks, is pure white, changing to light smoky blue on the last-named parts. The outer hair of the hips and thighs is tipt with grey instead of red, which gives these parts a hoary appearance, and this colour predominates on all the upper parts of the Society's two specimens, in which the fur is moreover much shorter and coarser, and the colours less brilliant and varied than in Mr. Royle's. The whole under surface of the body is of a smoky brown colour, without any intermixture of long silky hairs. The external colours of the body are, therefore, bright bay on the back, yellowish red on the sides of the body, white on the sides of the neck, hoary grey on the hips, and smoky brown on the throat, breast, and belly. The ears are pretty large and elliptical, their outer surface black; a stripe of the same colour runs down the front of the legs, both fore and hind; the soles of the feet are thickly covered with hair of a yellowish brown colour, except the balls of the toes, which are naked. The brush is large and well finished, of the same colour as the body throughout the greater part of its length, and terminated by a large white point.

Mr. Gray related a series of facts in reference to the habits of a *Cuckoo*, which appeared to prove that the female, though she leaves the eggs to be hatched by another bird, sometimes at least takes care of the young bird and feeds it after it leaves its nest, and teaches it to fly. They may explain how they are taught to migrate.

He also expressed some doubt respecting the eggs of *Cuckoos* being laid in the nest of *Granivorous birds*, and stated an instance where a chicken had been hatched under a *Pigeon*, that the *Pigeon* neglected it when it found that it would not eat the soaked peas, and eventually ejected it from its nest.

Mr. Gray then exhibited and explained a peculiarity in the structure of the ligaments of bivalve shells, and pointed out the peculiarity of some mactraceous shells which had this part, contrary to the general structures, inclosed in the cartilage pit, observing that this structure was found in his genus *Gnathodon*, and in a new genus,

which Mr. Gray had called at the British Museum *Mulinia*, of which he described five species; and he also stated the necessity for forming a new genus, of which *Mactra Sprengleri* may be regarded as the type.

Mr. Harvey, of Teignmouth, exhibited various fossils from Devonshire. Of these, sections in different directions had been made, and the surfaces highly polished. The structure was thus rendered beautifully apparent.

Mr. Harvey also exhibited various specimens of *Asterias* and *Ophiura* from the Devonshire coast, and explained the mode by which they had been prepared.

Mr. Gould brought under the notice of the Meeting several species of *Birds* from New South Wales, which he considered to be new to science, as they are not contained in the collection of the Linnean Society; nor, as far as he is aware, described in any publication. Mr. Gould embraced this opportunity to characterize and name ten species, and stated that at subsequent meetings of the Society he would bring forward the remainder of his collection.

Mr. Gould more particularly pointed out a species of *Petroica*; a new and interesting species of *Ptilonorhynchus*, allied to *Ptil. nuchalis*, and which he proposed to make the type of a new genus; a new species (belonging to the Society) of the genus *Calyptorhynchus*, which he compared with all the other members of the group then on the table, and described as *Calyptorhynchus Naso*; and four new species of the genus *Amadina*, Swains., which he named *Amadina cincta, ruficauda, modesta*, and *Castanotis*. The species are as follows, their characters, as usual, being given in the "Proceedings": *Petroica phœnicea; Amadina Castanotis, modesta, cincta*, and *ruficauda; Calodera maculata; Cracticus hypoleucus* and *fuliginosus;* and *Calyptorhynchus Naso*.

GEOLOGICAL SOCIETY.

Anniversary Meeting, Feb. 17*th,* 1837.—On the occasion of presenting the Wollaston Medals, the President, Charles Lyell, jun., Esq., F.R.S., addressed the Meeting in the following manner:

Gentlemen,

You have just learnt from the Report of the Council that they have this year awarded two Wollaston medals; one to Captain Proby Cautley of the Bengal Artillery, and another to Dr. Hugh Falconer of the Bengal Medical Service, for their geological researches and their discoveries in fossil geology in the Sub-Himalayan mountains. I shall now request one of our Secretaries, Dr. Royle, to take charge of these medals.

The President then addressed Dr. Royle:

Dr. Royle,

It will, I am sure, be most gratifying to you to be intrusted with the care of these testimonials of our regard for two gentlemen with whom you are connected by the ties of private friendship. The Geological Society awards these medals to Capt. Cautley and Dr. Falconer as an expression of the sympathy which they feel for

those who are so zealously labouring in a distant country to promote a common cause.

In the Address which I am now about to deliver to this Meeting, I shall have an opportunity of enlarging on the discoveries which these gentlemen have made in a region previously unexplored, at the southern base of the Himalaya between the Sutledge and the Ganges. I shall then speak of their perseverance and industry in examining the structure of the hills, and in collecting the remains of extinct quadrupeds and reptiles, and the talent displayed in their anatomical determination of new species and new types of organization. I shall now merely request that in forwarding these medals, the first which the Geological Society has sent to India, you will express to Capt. Cautley and Dr. Falconer the lively interest which we continue to take in their researches, and our ardent hopes for their future welfare and success.

Dr. Royle in reply expressed the high satisfaction he felt on being requested to take charge of the medals, which it would give him great pleasure to forward immediately to India. When in that country, he had had personal opportunities of witnessing the zeal and enthusiasm with which his friends had laboured, and the great difficulties which they had overcome when far separated from the scientific world, and without museums, books, or skilful naturalists to consult.

He was assured that these marks of attention so honourably conferred by the Geological Society on Capt. Cautley and Dr. Falconer, would not only encourage and stimulate them to fresh exertions, but inspire others among our countrymen in India with a desire to cultivate Geology and its kindred sciences.

It was afterwards resolved :—

1. That the thanks of this Society be given to Sir Philip de Malpas Grey Egerton, Bart., M.P., retiring from the office of Vice-President. Mr. Whewell and Mr. Murchison, by whom this motion was proposed and seconded, felt that they expressed only the sentiment of every Fellow of the Society, in declaring their deep regret at being prevented from including in the motion the name of Dr. Turner, who had been one of the Vice-Presidents, but whose loss the Society had then to deplore.

2. That the thanks of this Society be given to Sir Alexander Crichton, M.D., William John Hamilton, Esq., Viscount Oxmantown, and Lieut.-Col. W. H. Sykes, retiring from the Council.

On the close of the ballot the scrutineers reported that the following gentlemen had been duly elected the Officers and Council for the ensuing year:—

Officers.—*President,* Rev. William Whewell, M.A. F.R.S. : *Vice-Presidents,* Rev. W. Buckland, D.D. F.R.S. & L.S. Professor of Geology and Mineralogy in the University of Oxford; William Henry Fitton, M.D. F.R.S. & L.S. ; George Bellas Greenough, Esq. F.R.S. & L.S. ; Roderick Impey Murchison, Esq. F.R.S. & L.S. : *Secretaries,* Robert Hutton, Esq. M.R.I.A. ; John Forbes Royle, M.D. F.L.S.

Professor of Materia Medica and Therapeutics in King's College, London: *Foreign Secretary,* H. T. De la Beche, Esq. F.R.S. & L.S.: *Treasurer,* John Taylor, Esq. F.R.S.

Council.—F. Baily, Esq. Treas. R.S. F.L.S.; W. J. Broderip, Esq. F.R.S. L.S.; W. Clift, Esq. F.R.S.; Viscount Cole, M.P. D.C.L. F.R.S.; Charles Darwin, Esq.; Professor Daubeny, M.D. F.R.S. L.S.; Sir P. Grey Egerton, Bart. M.P. F.R.S.; H. Hallam, Esq. F.R.S.; Leonard Horner, Esq. F.R.SS. L.&E.; C. Lyell, jun. Esq. F.R.S.L.S.; Marquis of Northampton, F.R.S.; Sir Woodbine Parish, K.C.H. F.R.S.; Rev. Prof. Sedgwick, F.R.S. L.S.; Henry Warburton, Esq. M.P. F.R.S.

Address to the Geological Society, delivered at the Anniversary, on the 17th of February, 1837, by Charles Lyell, *Jun., Esq., President.*

Gentlemen,

You will have learnt from the Treasurer's Report that the finances of the Society are flourishing, and they would have appeared in a still more prosperous condition, had we not expended above 500*l.* within the year on our Transactions. Part of this sum has already been repaid by the sale of the volume just published, of which I may safely say that it yields to no preceding number in the value of its contents or the extent and beauty of its illustrations.

The total number of Fellows of the Society, exclusive of Honorary and Foreign Members, at the close of the year 1835, was 670; at the close of 1836, 709; being an actual increase, after deducting 14 for deaths, removals, and resignations, of 39 Fellows*.

We have to lament the loss of Dr. Henry, of Manchester, so highly distinguished as a chemist and philosopher, and who took a warm interest in the progress of our science. Our list of Foreign Members has been diminished by two deaths, those of Professor Hoffmann of Berlin, and Baron Férussac of Paris.

Professor Frederick Hoffmann was suddenly cut off in his 39th year, at the moment when the scientific world were impatiently expecting his account of the Geology of Sicily. You are probably best acquainted with him as the author of the great Geological Map of Western Germany, in which he made known the results of many years of patient and accurate research. This Map, published in 1829, was divided into twenty-four sheets, and was followed in 1830 by an Atlas containing sections, and a more general map on a smaller scale of the same country. In the same year the author's Geography and Geology of North-western Germany appeared†, which may be regarded as a commentary on the great map, comprising a description of the physical outline of the country, its mountains, valleys, plains, and river-courses, and a sketch of a portion of its geo-

* The return of the number of Fellows, and the deaths alluded to in this Address, refers exclusively to the year 1836, and not to the period intervening between the last and present Anniversary.

† Orograph. und Geognost. Verhältnisse vom Nordwestlichen Deutschland, 2 vols. Leipzig, 1830.

logical structure, embracing the transition and secondary rocks of the Hartz, Thuringerwald, and Lower Rhine. In the larger map all the tertiary and alluvial deposits are represented by one colour, the author having never entered upon the subdivision and classification of these formations. He had studied, however, the newer secondary formations, which were depicted by several distinct colours, and their history would have been included in the work above alluded to, had he not been interrupted by his tour in Italy and Sicily in 1830.

Among his other writings, I may enumerate an Account of Magdeburg, Halberstadt, and the adjoining territory, and various papers which will be found scattered through the journals of Poggendorff and Karsten, the Hertha, and other German periodicals. The only fruits which we as yet possess of the scientific expedition sent by the Prussian Government under Hoffmann's direction to Italy and Sicily, are some letters written by him during the journey, and an excellent Memoir on the Lipari Islands; and a valuable work by one of his companions, Dr. Philippi of Berlin, who published in Latin a detailed account of the recent testacea of Sicily, and the tertiary fossil shells collected in the course of the expedition*.

From Hoffmann's letters it clearly appears that the novelty of the volcanic and tertiary phænomena of Southern Italy and Sicily had made a deep impression on his mind. He had been astonished, on recognising the identity of the modern trap rocks of the Val di Noto with those of ancient date in Germany, and the no less striking similarity of the Siciliant ertiary limestones, containing recent shells to many calcareous secondary formations of northern Europe. The Lipari Islands afforded him a field for the examination of modern igneous rocks, and the slow effects of volcanic heat in modifying aqueous deposits. The picture which he has given of the fumeroles of the western coast of Lipari, the principal island of the group, is graphic and highly instructive. At St. Calogero numerous fissures are seen permeated by heated vapours which are charged with sulphur, oxide of iron, and other minerals, in a gaseous state. Here the tufaceous and other rocks are variously discoloured wherever the steam has penetrated, and are sometimes crossed with ferruginous red stripes, so as to assume a chequered and brecciated appearance. In one place a felspathic lava has been turned by the vapours into stone as white as chalk marl, in another, a dark clay has become yellow or snow-white, and these effects are not limited to a small space, but are seen extending for four miles through horizontal strata of tuff, which rise occasionally to the height of more than 200 feet. The greater part however of the alterations are referred to what are properly called extinct fumeroles, or the action of volcanic emanations which have now ceased, but which must at one period have resembled those of St. Calogero. Some of these

* Philippi, "Enumeratio Molluscorum Siciliæ tum viventium tum in tellure tertiaria fossilium, quæ in Itinere suo observavit Auctor." 280 pages 4to, and 12 lithographic plates, Berlin, 1836.

have produced veins of fibrous gypsum, calcedony, and opal, minerals which must have been introduced into the rents in a state of sublimation.

In some places there are tufaceous marls, regularly alternating in thin beds, with still thinner and countless layers of granular gypsum, the whole mass being again run through everywhere by irregular branching veins of silky fibrous gypsum. These strata, thus intersected, present a perfect counterpart to some of the secondary gypseous marls, both of the keuper and variegated sandstone formations in Germany*.

When reading the Professor's description of these phænomena, we share in the pleasure and surprise which he felt on comparing strata of high antiquity with others of so recent a date, and which, moreover, owe a portion of that resemblance to changes now daily in progress.

The writings of Baron Daudebard de Férussac were not devoted principally to Geology, but we are indebted to him for several memoirs, and among others for an Essay, published in 1814, on freshwater formations, with a catalogue of the species of land and freshwater shells which were then known to enter into their composition. Monsieur de Férussac contributed largely to the Geological section of the Bulletin Universel des Sciences Naturelles, a journal, of which he was the chief editor and original projector. This Bulletin had, for its object, to give a monthly analysis or brief abstract, usually unmixed with criticism, of the contents of all new publications in every department of science. The work was first carried on for a year on a smaller plan, and then assumed in 1824 its enlarged and permanent form, being divided into eight sections, one of which was devoted to Geology, Palæontology, and Natural History. A monthly number appeared regularly, on this and each of the other seven sections, the whole forming together a large octavo volume. In the organization and direction of this scheme, the Editor was indefatigable, and he succeeded in obtaining the co-operation of a great number of the most able and eminent writers. In announcing the original aim and scope of the undertaking, he laid stress on the difficulties under which men of science labour in procuring intelligence of new works, written in a great variety of languages in different parts of the world, and frequently buried in the voluminous and costly transactions of learned societies. He therefore expressed a hope that his Bulletin would serve as "a kind of telegraph" for the rapid conveyance of the earliest intelligence of inventions and discoveries, so as to prevent philosophers from wasting their time and money in slowly feeling their way to results already found out by others, and attaining with great labour the very points from which they might have started. The Geological section of the Bulletin was ably supported by MM. Boué, Brongniart, and other writers, and survived the other sections for some time,

* Liparischen Inseln, p. 41. Leipzig, 1832.

maintaining itself for seven years, till at length it was given up in 1831 for want of sufficient encouragement.

The works of Baron Férussac on Natural History, and especially Conchology, would deserve from me a fuller notice, if they were not irrelevant to the subject of this address.

HOME GEOLOGY.

I shall now commence my retrospect of the proceedings of the Society, during the last year, by considering those papers which have been devoted to the Geology of the British Isles. There is probably no space on the globe, of equal area, which has been so accurately surveyed as this kingdom; yet the most experienced geologists are now exploring several parts of it with the feeling that they are entering upon terra incognita. Not only do they find it necessary to trace out more correctly the limits of formations previously known, but also to introduce new groups of fossiliferous strata and new divisions, in districts before supposed to have been well investigated.

The carboniferous deposits which are alike interesting, in a scientific and economical view, have deservedly occupied of late the particular attention of many able geologists, and we have received communications on the subject from Mr. Murchison, Mr. Prestwich, Professor Sedgwick, and Mr. Peile. The observations of Mr. Prestwich relate to the coal-measures of Coalbrook Dale, and the formations immediately above and below them, together with the accompanying trap-rocks*.

There is perhaps no coal-field in the whole country of equal size in which the strata have been so much dislocated and shattered. Mr. Prestwich gives a detailed description both of the principal and minor faults, their direction, extent, inclination, breadth, and fall, and the difference of level produced by them in their opposite sides, which is sometimes slight, but sometimes amounts to 600 or 700 feet. In some instances the change of level is by steps or hitches, which, it is truly said, may be owing either to unequal resistance, or to a series of small dislocations. The walls of the fissures in the disjointed strata are sometimes several yards apart, the interval being filled with the debris of the strata. In other places they are in contact. In this last case it is particularly remarked that the surface of the ends of the fractured beds of coal and shale is shining and striated. You are aware that this appearance has usually been attributed, and I believe rightly, to the rubbing of the walls of the rent one against the other, the lines of the polished and striated surfaces indicating the direction of the motion, but I have lately seen it objected to this theory, that the striæ are not always parallel, but often curved and irregular, and that the earthy contents of veins and faults often present the same glittering and striated faces, or slickensides as they have been called. I

[* An abstract of Mr. Prestwich's paper will be found in Lond. and Edinb. Phil. Mag., vol. ix. p. 382.—Edit.]

am familiar with the fact, and have always inferred that the movements were irregular and complicated, occasionally changing their direction, and that even when uniform, they may have acted unequally on materials varying in hardness and pliability. It is much to be desired that scientific travellers who visit countries shaken by earthquakes would observe with minute care all the phenomena attending the fissuring of rocks and buildings. I have been informed by an eye-witness of one of the late minor earthquakes in Chili, that the walls of his house were rent vertically, and made to vibrate for several minutes during each shock, after which they remained uninjured and without any opening, although the line of the crack was still visible. On the floor, at the bottom of each rent, was a small heap of fine brickdust, evidently produced by trituration. In such instances it would be desirable to obtain fragments of the rent building, and to compare them with the walls of natural fissures.

In his examination of the fossils of the coal-measures, Mr. Prestwich has shown that beds containing marine remains alternate with others in which fresh-water shells and land plants occur, appearances which he attributes to the flowing of a river, subject to occasional freshes, into the sea, rather than to repeated changes in the relative level of land and sea.

It is certainly the safer course to incline to this hypothesis whenever there are no unequivocal signs, as in the Purbeck strata in Portland, of land plants having become fossil on the very spots where they grew. For although there may be many river deltas like that of the Indus, where the land is subject to be alternately upheaved above, and then let down below the waters of the sea, yet such oscillations of level must be considered as exceptions to the general condition of the earth's surface near the mouths of rivers at any given period. Even in a case like the delta of the Indus, both the causes above alluded to may be expected to cooperate in producing alternate fluviatile and marine strata; for in the long intervals between great movements of the land, the river will annually advance upon the sea with its turbid waters, and then retreat again as the periodical flood subsides, and the salt waters, after being driven back for a time, will reoccupy the area from which they have suffered a temporary expulsion.

In the conclusion of his valuable paper, Mr. Prestwich observes that the carboniferous strata of Coalbrook Dale must once have been entirely concealed under a covering of new red sandstone, and they owe their present exposure partly to those movements which have shattered and elevated the coal measures, and partly to extensive denudation. It is natural therefore to inquire how many other coal-fields may still lie buried beneath the new red sandstone of the adjoining district.

In relation to this point of great practical importance, Mr. Murchison formerly offered some conjectures, when speaking of the probable passage of the 10-yard coal of the Dudley field beneath the new red sandstone, which there flanks it on the east and west. That

geologist now informs us that his conjectures have been verified, and that at Christchurch, one mile beyond the superficial boundary of the coal-field, the 10-yard and other seams have been reached by borings carried down to the depth of nearly 300 yards. Adverting to this discovery, he directs attention to the possible extension of other carboniferous tracts beneath the surrounding new red sandstone of Shropshire, Worcestershire, Staffordshire, and other central counties.

It is clear that these geological considerations must be duly weighed by those who speculate on the probable future duration of British coal, according to the actual or any assumed rate of consumption.

Mr. Murchison, in describing the Dudley and Wolverhampton coal-fields*, informs us that he has not yet found any fossil remains of decidedly marine origin, like those observed by Mr. Prestwich in Coalbrook Dale. The shells seem to be all of fresh-water genera, and the *Megalichthys Hibberti*, and other fish occurring at Dudley, of species identical with those of the coal measures of Edinburgh, may have inhabited fresh water.

The same author has coloured on an Ordnance Map the superficial area of the Silurian rocks connected with the coal-fields above mentioned, and has shown that the Lickey quartz rock between Bromsgrove and Birmingham, of which the geological position has remained hitherto uncertain, is in fact nothing more than altered Caradoc sandstone, a member of the lower Silurian group. The same appears as a fossiliferous sandstone in one district, while in another it passes into a pure quartz rock, a modification attributed to the proximity of underlying trap, for analogous changes have been seen at neighbouring points where the absolute contact of the sandstone with the trap is visible.

We are also indebted to Mr. Murchison for some interesting remarks on the dislocations of the strata in the neighbourhood of Dudley, and particularly for a description of some dome-shaped masses, from the centre of which the beds have a quâquâversal dip. He speculates on the probable dependence of these phænomena upon the protrusion of volcanic matter from below, at points where it has been unable to find issue. It would, I think, have been more satisfactory, if, in confirmation of his theory, some natural section of one of these dome-shaped masses could be pointed out, where not only a nucleus of trap was apparent, but could be shown to have taken up its actual position in a soft or fluid state. Even if we should find in some instances a subjacent central mass of trap, porphyry or granite, not sending out veins or altering the strata, the folding of the beds round such a protuberance might admit of an explanation like that suggested by Dr. Fitton. He has supposed a set of yielding horizontal strata to be pressed upon by a sub-

*[The abstract of Mr. Murchison's memoir on these coal-fields appeared in Lond. and Edinb. Phil. Mag., vol. ix. p. 489.—Edit.]

jacent hill or boss of hard rock, in which case the effect of upward pressure might resemble that seen, on a small scale, in the paper of a bound book, where a minute knob in one leaf has imparted its shape to a great number of other leaves without piercing through them*. Whatever hypothesis we favour, it is essential to observe that such hills as the Wren's Nest near Dudley, and others of similar ellipsoidal forms and internal structure, do not correspond to the type of volcanic hills, such as Etna, Mount Dor, or the Cantal. In both cases there may be an approach to a cone, and the beds may dip everywhere outwards from a common centre; but, in the volcanic mountain, the beds having an outward dip, thin off as they approach the base or circumference of the cone, which is not the case in inclined beds composing the hills alluded to in the neighbourhood of Dudley: nor in the last-mentioned instances do the lowest or subjacent rocks crop out round the circumference of the cone, as happens in the instance of the volcanic eminences before alluded to, where the granite of the country round Mount Dor, the fresh-water beds and mica schist in the Cantal, the marine deposits around Mount Etna in Sicily,—each appear at the surface as soon as we have left the slope of the cone, and advance upon the surrounding low country.

In attempting to explain the principal transverse faults of the Dudley coal-field, Mr. Murchison refers frequently to the theoretical principles expounded by Mr. Hopkins in his Researches in Physical Geology, a paper printed in the 6th volume of the Transactions of the Cambridge Philosophical Society†. Mr. Hopkins has there endeavoured to develop, by reasoning founded on mechanical principles, and by mathematical methods, the effects of an elevatory force acting simultaneously at every point, beneath extensive portions of the crust of the earth. He is aware that in nature such a force must usually act under complicated conditions, so as to produce irregular phænomena; but he observes that in order to have a clear conception of the manner in which it would operate in producing movements and dislocations, it is useful to assume certain simple conditions to which mathematical investigations may be applied. When we have deduced in this manner some results free from all uncertainty, these may serve as standard cases to which the geologist may refer more complex problems. Thus for example, a portion of the earth's crust may be assumed to be of indefinite length, of uniform depth, and bounded laterally by two vertical parallel planes, beyond which the disturbing force does not extend. It is then supposed that a quantity of subterranean vapour or melted rock, existing at a certain depth, is expanded by heat so as to elevate

* Dr. Fitton, Geol. Trans. 2nd Series, vol. iv. p. 244.

† [Mr. Hopkins's "*Abstract of a Memoir on Physical Geology; with a further Exposition of certain points connected with the subject,*" appeared in Lond. and Edinb. Phil. Mag., vol. viii. p. 227. *et seq.* A discussion also of certain parts of the subject, by Dr. Boase and Mr. Hopkins, will be found in vol. ix. pp. 4, 14, and 171, *et seq.*—Edit.]

the superincumbent mass, the resulting fissures in this mass may then become matters of calculation. According to Mr. Hopkins, rectilinear lines of dislocation will give rise to a set of longitudinal parallel fissures, and simultaneously to others precisely at right angles to them; whereas in conical elevations, the fissures will diverge from a centre. If the general axis of elevation be curvilinear, the longitudinal fissures preserving their parallelism with it will be also curvilinear, while the transverse fissures being perpendicular to the former at their points of intersection will no longer be parallel.

To return from this digression, I must now recall your attention to other papers relating to the carboniferous deposits of England. The coal-measures of the north-western coast of Cumberland have been examined by Prof. Sedgwick and Mr. Williamson Peile, who have described the Whitehaven and other fields in great detail, illustrating their account with a map and sections*. The recorded observations in numerous sinkings and borings, both in relation to the succession of the strata and to the complicated faults which intersect them, would have been involved in hopeless confusion, if they had simply consisted of a statistical collection of facts attested by miners; but in this paper, Professor Sedgwick, aided by Mr. Peile's practical and scientific knowledge, has compared the different sections and generalized the phænomena, giving unity and consistency to the whole, throwing the strata into distinct groups, and referring the several faults to different movements to which successive periods of time may be assigned.

In connection with these recent contributions to the history of our carboniferous strata, I am happy to mention the excellent volume lately published by Professor Phillips, forming the second part of his Illustrations of the Geology of Yorkshire. It is almost entirely devoted to a description of the carboniferous or mountain limestone of Yorkshire and the North of England, a subject already admirably treated in some papers read before this Society by Professor Sedgwick, particularly in his account of the carboniferous chain from Penigent to Kirkby Stephen†. As these geologists had separately explored the same ground, it is satisfactory to perceive that the leading divisions which they have proposed for the classification of the mountain limestone and associated strata, agree in every essential point. Mr. Phillips has described the physical geography of the district occupied by these rocks, their lithological character, stratification, jointed structure, and the most remarkable faults which affect them, especially those which have been called the great Penine and Craven faults. He also treats of the trap dykes which cut through the limestone, and discusses the probable epochs of the displacement of the strata, judiciously pointing out the difficulties unavoidably opposed to the

* [Prof. Sedgwick and Mr. W. Peile's paper was noticed in Lond. and Edinb. Phil. Mag., vol. ix. p. 501.—Edit.]

† Trans. Geol. Soc. 2nd Series, vol. iv. part 1, p. 69.—1835.

rigorous determination of the date of such dislocations. A large and very valuable portion of the work is filled with descriptions and plates of organic remains, especially of the brachiopodous and cephalopodous mollusca. Most of the species of these classes were probably inhabitants of the deeper parts of the sea, but there are fossil shells in the mountain limestone, which the author supposes to have lived near the shore, and belonging to genera formerly regarded as foreign to the carboniferous limestone, such as Isocardia, Nucula, Pecten, Patella, Turritella, and Buccinum. Many species of Zoophytes and Crinoidea are also described and figured in this excellent monograph.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

Feb. 13.—A meeting of the Cambridge Philosophical Society was held on Monday evening, Dr. Clark, the president, in the chair. Read—memoir, &c., by Prof. Rigaud of Oxford, on the proportion of land and water on the surface of the terraqueous globe; memoir by Prof. Challis, on the law of decrease of temperature in ascending in the atmosphere; memoir by Mr. Kelland, on the transmission of light through crystallized media.

Feb. 27.—A meeting of this Society was held on Monday evening, the president, Dr. Clark, being in the chair. A paper by Mr. Warren, of Jesus College, was read, on the algebraical sign of the perpendicular, drawn from a given point to a given straight line.—Mr. C. Darwin exhibited various specimens of rocks, collected by him in a voyage round the world, made in His Majesty's ship Beagle, Capt. Fitzroy, and occupying five years. These specimens were—tubes of fused sand (produced by lightning?) found near the Rio Plata; a white calcareous incrustation alternately formed and removed on the rocks of Ascension Island by a periodical change in the direction of the swell; a black incrustation formed by the spray on the tidal rocks at Ascension; a white hard calcareous rock formed rapidly at Ascension; a recent calcareous formation indurated by the contact of lava at St. Jago, one of the Cape de Verde islands.—Afterwards Mr. W. W. Fisher gave an account of a case of Spina Bifida, accompanied by some physiological and pathological researches on the accumulation of fluid in the ventricles of the brain. He came, from the facts he brought forward, to the following conclusions:—That as there exists a correspondence between the development of the central part of the nervous system and the organs destined to protect it, (the development of the osseous portion being subordinate to that of the nervous, by reason perhaps of its subsequent formation,) so the organic characters of the parts contained, and the peculiar construction of the parts containing, require that a reciprocity of adaptation should afterwards exist between them;—That the pia mater, except where it is united with the arachnoid so as to present the generic character of a serous membrane, possesses a faculty of secreting a fluid, the quantity of which is limited by the degree of resistance offered by the inclosing parts, and that it is thereby calculated, by its particular arrange-

ment in the central cavities of the brain and cerebellum, to effect the purposes of temporary or permanent adaptation;—That although congenital hydrocephalus may, in the first instance, be referred to certain conditions of the development of the encephalon and its envelope, these conditions being associated with, or rather expressive of, the special or general plastic powers of the economy, yet the inordinate accumulation of fluid in the ventricles of the brain may also be partly attributed, at a later period, to the faculty of the pia mater before specified, or to any obstruction to the flow of venous blood through the venæ Galeni, or the straight sinus;—That the ventricular fluid does not communicate with the sub-arachnoid cavity of the spine, as described by M. Majendie, and that the inferences which he draws with regard to the movement of the fluid, from the experiments detailed by him, are fallacious, inasmuch as, by interfering with the integrity of the organs containing the central parts of the nervous system, he thereby removes the most important condition by which the osseous protection is normally characterized, and exposes the parts contained to the direct influence of atmospheric pressure.

March 13.—Dr. F. Thackeray, V.P., in the chair. Read—Supplement to a memoir on the transmission of light in crystallized media, having reference particularly to the laws of biaxal crystals; by Mr. Kelland of Queen's College. Memoir on the laws of fluid motion, by the Rev. S. Earnshaw, of St. John's College. Medical Statistical Report of Addenbrooke's Hospital for the year 1836.

Mr. Whewell gave an account, illustrated by diagrams, of some of the recent results of his researches on the tides. It was stated that the diurnal inequality, or difference of the two tides on the same day, follows very curious and unexpected laws, which the author has ascertained by means of a series of calculations, executed by Mr. Dessiou and Mr. D. Ross of the Admiralty. This inequality is regulated by the moon's declination, and the exactness with which it conforms to a rule, depending on the declination, is very remarkable at some places, as Plymouth and Sincapore. But the declination is followed by the corresponding effect, at intervals of time which are different at different places; the interval being, half a day or a day on the coast of the United States; two days on the coast of Spain and Portugal; four days at Plymouth; five at Liverpool; and apparently *twelve* days at Leith. Also the amount of this inequality is very great in some cases, for instance, in the Indian Seas. At Sincapore it is so large that one tide is almost obliterated; and at other places, as King George's Town, in Australia, this obliteration takes place entirely, and there is only one tide in twenty-four hours at certain periods of the lunation.

FRIDAY EVENING PROCEEDINGS AT THE ROYAL INSTITUTION.

January 20, 1837.—Mr. Faraday on Mossotti's reference of electrical attraction, the attraction of aggregation, and the attraction of gravitation to one cause. Signor Mossotti assumes one electric fluid hav-

ing idio-repulsive powers; the particles of matter are also assumed as mutually repulsive, but matter and electricity are considered as mutually attractive. All these forces are inversely as the square of the distance, but the second is not quite so strong as the first and third. These assumptions being made the law of universal gravitation, all the varieties under which statical electricity presents itself, and the general condition of aggregation in solids and fluids flow as necessary consequence. For the more complete account we refer to the original paper by Mossotti in the Third Part of the Scientific Memoirs, &c. p. 448.

Jan. 27. Mr. Brande on Embossing. The illustration consisted chiefly of the embossing of soft materials, as paper, wood, leather, &c., and was exemplified by the machinery of Mr. De la Rue.

Feb. 3. Dr. Grant on the Development of the Glandular System in the animal kingdom compared with that in man.

Feb. 10. Dr. Ritchie on the Velocity of Sound, and the discrepancy existing between theory and the results of observation.

Feb. 17. Mr. Faraday on Dr. Marshall Hall's Reflex Function of the Spinal Marrow, and on Mr. Cowper's Parlour Printing Press.

Feb. 24.—Mr. Cowper on type and stereotype founding.

March 3.—Mr. Woodward, A demonstration by the oxy-hydrogen blowpipe and lime, and also by models and experiments, of the general laws and properties of polarized light.

March 10.—Mr. Wilkinson on bronze, and on various combinations of iron and steel to produce the varieties of Damascus.

March 17.—Mr. Faraday on Mr. De la Rue's mode of applying sulphate of copper to the exaltation of the powers of a *common* voltaic battery.

LXII. *Intelligence and Miscellaneous Articles.*

FOSSIL INFUSORIA USED FOR FOOD.

IT appears from a letter addressed to Ehrenberg by Prof. Retzius of Stockholm, that the mineral substance commonly called Bergmehl, mountain-meal, described and analysed by Berzelius, and in which he found silex, animal substance, and crenic acid, is sometimes eaten in Lapland in times of famine, when the Laplanders mix it with ground corn and bark, to make their bread. It was used thus in the district of Degerfors in 1833, and is superstitiously considered as a gift of the great spirit of forests. Retzius adds that he has discovered in the Bergmehl, nineteen different forms of Infusoria with siliceous shields, the mineral being wholly composed of them, and that the analogy which he supposed to exist between it and the Bergmehl of Franzensbad seems to be well founded*.

PALÆONTOLOGY.

Organic Forms of certain Minerals.—Prof. Ehrenberg lately read to the Academy of Berlin the following note on the organic forms

* For a description of the Infusoria contained in this deposit, see M. Ehrenberg's memoir on Fossil Infusoria, in Scientific Memoirs, Part III. p. 400.

which he had observed, with the help of a microscope, in earthy and soft minerals.

"An exact microscopical analysis, several times repeated, of upwards of a hundred minerals, of different groups, showed me—

"1. That chalk, both white and coloured, consists of small elliptical bodies, flat and symmetrical, or their fragments,—bodies which vary in magnitude from $\frac{1}{480}$ to $\frac{1}{192}$ lin., and are formed of concentric articulated rings.

"2. That the *Calcaire cotonneux*, or *Bergmilch*, and the *Calcaire incrustant*, or *Kalkguhre*, consist of small articulated needles, straight and rigid, often collected together in fascicles, and in which the articulations or grains (elementary particles, not atoms,) exhibit a tendency to form a spiral.

"3. That the porcelain earth of the Aue and Calle (true Kaolin, in which are likewise found fragments of feldspath,) consists also of round bodies, larger, to the size of $\frac{1}{36}$ lin., regular, similar to those of the chalk, but discoidal or in their fragments.

"4. That the *Meerschaum* and *Bergleder* consist of threads or very finely articulated nets, more or less interlaced or felted (*feutrés*) and flexible, the articulations of which are constantly of a uniform size.

"5. That the mixed earths or rocks, as the potters' clay, the *glaises*, the pseudo-meerschaum, also exhibit, on microscopical analysis, very curious facts of the same kind.

"6. That even crystallized quartz and mica, as well as some other minerals, present a granulated appearance of great regularity, either without their outer surface of fracture undergoing any previous preparation, or after having been warmed or heated to redness.

"7. That by artificial means, such as a red heat, siliceous and argillaceous substances may be transformed (by the polarization of the elementary parts, which may be compared to the cellular tissue of plants,) into a tissue or felt, composed of articulated spiculæ. Nature exhibits this effect in the Meerschaum, and art produces it in the manufactory of porcelain and the slag of intense furnaces." —*L'Institut*, No. 194.

PYROPHORI OF EASY PREPARATION.

It is well known that when $2\frac{1}{2}$ parts of pure tartaric acid, deprived of its water of crystallization, are quickly mixed in a dry capsule with 8 parts of peroxide of lead, perfectly dry and reduced to powder, ignition very soon occurs throughout the mass, which is very vivid and of long duration. This fact, first mentioned by Walker, would lead to the supposition that other organic substances would undergo similar reaction with peroxyd of lead; and this has been verified by the experiments of M. Bœtliger. On experimenting with the oxalic and citric acids, he found that the action of the former on the peroxyd of lead was more rapid, and perhaps stronger, than that of tartaric acid; while that of citric acid was rather weaker. Thus, on mixing together $5\frac{1}{4}$ parts of peroxyd of lead, and 1 part of oxalic acid dried in hot air, or containing 19 per cent. of water,

almost instantaneous ignition of the mass occurs; but it continues for a much shorter time than with the tartaric acid, because the oxalic acid contains less carbon. In order to obtain a pyrophorus with citric acid, 1 atom of citric acid, previously fused and kept some time in fusion, then dried and pulverized, must be promptly mixed with 2 atoms of peroxyd of lead at the temperature of 73° Fahr. The ignition of the whole mass is almost as vivid, and continues for as long a time as with tartaric acid. Minium, litharge, and carbonate of lead, mixed with tartaric acid, yield also, according to M. Bœtliger, pyrophori, but not so good as those yielded by the pure oxyd.—*L'Institut*, March 1, 1837.

NOTICE OF M. MOSSOTTI'S MATHEMATICAL RESEARCHES RELATIVE TO THE LAWS OF MOLECULAR ACTION.

A translation of the memoir by M. Mossotti "On the forces which regulate the internal constitution of bodies," in which he has embodied the results on this subject which he has hitherto obtained, has already appeared in Part III. of the "Scientific Memoirs." As, however, the principal result of his labours,—the mutual identification of the attractive forces of electricity, aggregation and gravitation,—constitutes one of the most remarkable discoveries of the present æra in science, we think it desirable to notice it in the Philosophical Magazine, as a matter of reference.

While reflecting on the Franklinian hypothesis for explaining the phænomena of statical electricity, as reduced by Æpinus to the form of a mathematical theory, and with the addition subsequently made by Coulomb, proving that electrical attractions and repulsions are regulated by the law of the inverse ratio of the square of the distance, M. Mossotti conceived the idea, that if the molecules of matter, surrounded by their atmospheres, attract each other when at a greater, and repel each other when at a less distance, there must be between those two distances an intermediate point at which a molecule would be neither attracted nor repelled, but would remain in steady equilibrium; and that it was very possible that this might be the distance at which it might be placed in the composition of bodies. Learning subsequently that the attention of geometers had recently been particularly directed to the molecular forces, as being those which may lead us more directly to the knowledge of the intrinsic properties of bodies, he was thus led to recall his ideas on the subject, and to set about subjecting them to analysis, and he has submitted to the judgement of philosophers, in the memoir here referred to, the results of his first investigations. Of the contents of this memoir the following extracts may be regarded as a summary.

"I have supposed that a number of material molecules are plunged into a boundless æther, and that these molecules and the atoms of the æther are subject to the actions of the forces required by the theory of Æpinus, and then endeavoured to ascertain the conditions of equilibrium of the æther and the molecules. Considering the æther as a continuous mass, and the molecules as isolated bodies, I found that if the latter be spherical, they are surrounded by an atmosphere, the density of which decreases according to a function of the distance

which contains an exponential factor. The differential equation which determines the density being linear, is satisfied by any sum of these functions answering to any number of molecules. Whence it follows that their atmospheres may overlay or penetrate each other without disturbing the equilibrium of the æther. Proceeding in the next place to the conditions of equilibrium of the molecules, I observed that, for a first approximation (which may be sufficient in almost all cases), the reciprocal action of two molecules and of their surrounding atmospheres is independent of the presence of the others, and possesses all the characteristics of molecular action. At first it is repulsive, and contains an exponential factor, which is capable of making it decrease very rapidly: it vanishes soon after, and at this distance two molecules will be as much indisposed to approach more nearly as they would be to recede further from each other; so that they would remain in a state of steady equilibrium. At a greater distance the molecules would attract each other, and their attraction would increase with their distance up to a certain point, at which it would attain a maximum: beyond this point it would diminish, and at a sensible distance would decrease directly as the product of their mass, and inversely as the square of their distance."

"To apply the formulæ which we have found, for the purpose of presenting molecular action, to the phænomena of the interior constitution of bodies, requires methods of calculation which are not yet developed, and which must become still more complicated when the arrangement of the molecules, their form and their density, are taken into consideration. I have thought it advisable however, in consideration of the use to which it might be applied by able geometers, not to postpone the publication of this mode of viewing molecular action. It is a subject which appears to me entitled to the greatest attention, because the discovery of the laws of molecular action must lead mathematicians to establish *molecular mechanism* on a single principle, just as the discovery of the law of universal attraction led them to erect on a single basis the most splendid monument of human intellect, *the mechanism of the heavens.*"—*Scientific Memoirs*, Part III. p. 450.

IODAL.

M. Aimé has sent to the Academy of Sciences a new compound which he considers as analogous to chloral, and which he has named *iodal*, because iodine performs the same function in it as chlorine does in chloral.

This compound was obtained by causing iodine to act upon nitric alcohol [*alcool nitrique*]. By allowing the liquor to remain for some days it was replaced by a fluid which was of a red colour and heavier than water. The colour was owing to excess of iodine, and it eventually disappeared spontaneously. In this way the iodal was obtained nearly pure, except that it retained a little nitric alcohol and nitrous æther, from which it is easy to free it.

This substance when pure is nearly colourless. It has a sweet taste; its odour is somewhat æthereal. When poured on a red-hot

coal it yielded white fumes, which strongly affected the eyes. Sulphuric acid decomposes it, and converts it into iodoform. This process is equally applicable for procuring bromal and chloral, which may be readily obtained by heating the solutions.—*L'Institut*, Feb. 1st, 1837.

ON THE OXIBROMIDES AND SOME OTHER COMPOUNDS OF TUNGSTEN.

M. Bonnet states that he has obtained two oxibromides of tungsten by passing the vapour of bromine over tungstic acid mixed with charcoal and strongly heated. At a red heat, with a moderate current of bromine, an oxibromide was obtained, which yielded

Oxygen	6·03	
Bromine..................	48·00	
Tungsten	45·97	—100·

The composition of which gives $W^2 O^5$, $W^2 Br^{10}$, which is equivalent to an atom of blue oxide of tungsten and one atom of bromine. At a higher temperature than in the preceding case, and with the bromine passing more rapidly, a second oxibromide of tungsten was procured, which yielded

Oxygen	3·0	
Bromine	60·	
Tungsten	37·	—100·

The formula of which is $W O^3$, $W^2 Br^{12}$, which is equivalent to an atom of tungstic acid with 2 atoms of perbromide of tungsten, and which ought to be called tungstate of perbromide of tungsten.

Passing chlorine over the same mixture, instead of bromine, an oxichloride of tungsten was obtained, corresponding to the tungstate of perbromide of tungsten; this tungstate of perchloride of tungsten yielded

Oxygen	4·8	
Chlorine	40·0	
Tungsten	55·2	—100·

This composition corresponds with the formula $W O^3$, $W^2 Ch^{12}$, tungstate of perchloride of tungsten.

The tungstates of perbromide and perchloride of tungsten act upon water as indicated by their composition, but the case is not the same with the oxibromide $W^2 O^5$, $W^2 Br^{10}$. The analysis of these compounds was very difficult to perform.—*L'Institut*, Feb. 8, 1837.

ON CHLOROFORM AND CYANOFORM.

M. Bonnet obtained with great facility a large quantity of chloroform by heating chloride of lime and acetate of lime in an earthen retort. It is purified by precipitating the liquor with water, then distilling the lower stratum of the liquid, which is chloroform, from chloride of calcium.

Prussian blue or cyanide of mercury was substituted for chloride of lime, and a liquid was obtained which M. Bonnet supposed to be

cyanoform; it is purified by distilling it from chloride of calcium; by this a colourless soluble liquid is obtained, which does not take fire by the taper, which has a strong smell of hydrocyanic acid and tobacco smoke; it is quite neutral, soluble in water, alcohol and æther; potash does not readily act upon it. If the operation is well conducted, that is to say, if the heat be gradually raised, cyanoform and water only are obtained, without any trace of acetone, acetic or hydrocyanic acid, for the liquor is not acid, and contains no acetone, since it is not combustible; but if a drop of acetone be added to it, and it be then inflamed, the acetone burns.—*L'Institut*, Feb. 1837.

ANALYSIS OF SILK.

M. Mulder of Rotterdam remarks, that the only analysis of raw silk which we possess is that by Roard, inserted in the 65th volume of the *Annales de Chimie*, which, according to the present state of science, is incomplete and unsatisfactory.

To analyse silk M. Mulder subjected some yellow raw silk from Naples, and white raw silk from Amasieh in the Levant, to the successive operation of boiling water, absolute alcohol, and acetic acid, and he examined each of these solutions for the substances which they might contain.

The cold water dissolved a portion of the colouring matter of the yellow silk; the solution contained gelatine and albumen, as well as some cerine; in the alcohol there were colouring matter, resin, and a solid fatty matter. The æther dissolved only a certain quantity of colouring matter and resin which had been partly taken up by the alcohol. As to the acetic acid, the substance which it dissolved had all the appearance of albumen. The residue insoluble in this acid M. Mulder considered as the pure filamentous part of the silk. The residue obtained by the evaporation of the water, mixed with a little alcohol, then with æther, gave a little cerine. Both silks when distilled with dilute sulphuric acid, yielded an acid liquor, to which the author gave the name of *bombic acid*, already employed by some authors.

The quantities of the several substances obtained from each kind of silk were as under:

	Yellow Silk.	White Silk.
Filamentous matter	53·37	54·04
Gelatine	20·66	19·08
Albumen	24·43	25·47
Cerine	1·39	1·11
Colouring matter	0·05	
Resinous and fatty matter ..	0·10	0·30
	100·00	100·00

Journal de Chimie Médicale, Jan. 1837.

FOSSIL MAIZE.

M. Warden announced to the Academy that he had received from Philadelphia some specimens of maize which he supposes to

be fossil. This maize is in isolated grains, sometimes agglomerated but without any apparent order; they were found in the state of Kentucky, in a district of alluvium, at a depth of five or six feet, in layers from eight to ten inches thick, and extended from four to five miles along the Ohio and its tributary the Fish Creek, 25 miles below Wheeling.—*Journal de Pharmacie*, Jan. 1837.

VEGETATION IN A SOLUTION OF ARSENIC.

M. Gilgenkrantz has seen a plant of the genus *Leptomitus*, or *Hygrocrocis*, form in a solution of arsenic. This observation, communicated by M. Bory St.-Vincent, proves that arsenic, a substance so very poisonous, and supposed to be destructive to all organized bodies, is however favourable to the vegetation of some plants. M. Bory St.-Vincent mentioned on this occasion that M. Dutrochet had observed about ten years ago the development of a similar plant in a solution of acetate of lead.—*Ibid.*

INDIGO.

Sulphindylic acid.—Analogy of alcohol and indigo, considered in their combination with sulphuric acid.

M. Dumas read a paper on indigo. This chemist repeated the analysis of indigo, and has obtained precisely the same results as those he arrived at fifteen years back. His analysis gives for the composition of indigo:

Carbon	7·30	
Hydrogen	4·0	
Nitrogen	10·8	
Oxygen	12·2	——100·0

The author afterwards endeavoured to determine the nature of the compound which is formed by the action of sulphuric acid upon indigo. It is known that sulphuric acid has the property of dissolving indigo, and of receiving a blue colour from this solution. M. Berzelius had considered this combination as a kind of emulsion. M. Dumas, on the contrary, supposes it to be a compound analogous to sulphovinic acid; he calls it for this reason sulphindylic acid; it results from a combination of two atoms of sulphuric acid with one atom of indigo.

This acid forms with potash a salt soluble in water, crystallizing in fine silky lamellæ, of a very deep blue: it produces with baryta a salt not very soluble in cold water, but more so when heated. The analysis of these two salts has shown that the formula for indigo is $C^{32}\ H^{10}\ AZ^{2}\ O^{2}$, and that the sulphindylic acid should be represented by $2\ SO^{3}+C^{30}\ H^{10}\ AZ^{2}\ O^{1}$; adding to this formula one atom of a base we have that of the sulphindylates.

It is known that by frequently treating indigo with sulphuric acid a purple matter is formed which is very difficult to isolate from the blue matter. M. Dumas calls this combination sulphopurpuric acid; it is represented by two atoms of indigo and two atoms of sulphuric acid, or of one atom of sulphindylic acid and one atom more of indigo: it forms with potash a purple salt, soluble in water.

White Indigo.—M. Dumas analysed the white matter into which indigo changes when exposed to the action of alkalis and disoxygenizing bodies; he found the composition the same as indigo, with the difference that the white indigo contains nearly two atoms more hydrogen. This makes it a hydruret of indigo, and not a deoxygenized indigo as is generally admitted.

Anilic acid.—This name was given by M. Dumas to an acid formerly called indigotic, and obtained by acting on indigo with nitric acid. This acid has not the same radical as indigo; it is represented by $C^{28}\ H^{8}\ AZ^{2}\ O^{9}$, and is anhydrous.

Picric acid.—This is the last product of the action of nitric acid on indigo, generally known by the name of Welter's bitter. It is composed, according to M. Dumas, of $C^{24}\ H^{4}\ AZ^{6}\ O^{1}i$.

M. Dumas thinks that an oxide of azote enters into its composition.—*Journal de Pharmacie*, Jan., 1837.

ON SOME OF THE PROPERTIES OF PER-IODIC ACID.

M. Bengiegser obtains this acid by decomposing the periodate of lead by dilute sulphuric acid and the application of heat, carefully avoiding any excess of sulphuric acid, as it often, when in excess, altogether prevents the crystallization of the per-iodic acid. When the decomposition is effected, and as soon as the precipitate is deposited, the solution must be poured off, as, by filtration, even at common temperatures, the acid is apt to be decomposed into iodic acid; this solution is then to be evaporated at a gentle heat until it is completely dry and crystallized.

This acid is colourless, and its crystalline form appears to be an oblique rhombic prism. In its crystallized state, when heated to 266° Fahr., it fuses without decomposing, and by cooling again crystallizes; at 324° Fahr. it loses its water of crystallization, and at about 370° Fahr. it is decomposed into iodic acid, with a rapid disengagement of oxygen gas. This acid is deliquescent, but may be kept solid over sulphuric acid. The crystallized acid is soluble in alcohol and æther, and the solutions diluted seem to suffer no change by ebullition. An aqueous solution of this acid heated with phosphorus forms oxide of phosphorus and phosphoric acid. The crystallized acid, heated with phosphorus, explodes violently, with the formation of oxide of phosphorus. In both cases the acid is completely deprived of its oxygen, and its iodine is set free. The aqueous solution of the acid exerts no action upon sulphur, even when they are boiled together. The action of per-iodic on the tartaric, formic, oxalic, and acetic acids is analogous to that exerted by iodic acid on these bodies, carbonic acid being formed and iodine set at liberty.

Most of the metals are oxidized by an aqueous solution of per-iodic acid. At common temperatures it acts on zinc filings, and, when excess of zinc is employed, the acid is completely reduced, giving rise to iodine and oxide of zinc. With copper filings it forms iodate of copper, which is precipitated in white flocks. Iron is converted

into deutoxide (*oxide ferrosoferrique*) and mercury into protoxide, whilst tin and lead are very slightly acted upon by this acid.

When per-iodic acid is neutralized by carbonate of soda, and precipitated by neutral nitrate of barytes, the resulting liquid is acid; this is also the case with the neutral salts of lead and lime, indicating the precipitation of a subsalt. Per-iodate of lead is white, but becomes yellow by heat, owing to its losing water. Similar phænomena occur with the proto- and bi-salts of mercury, the former changing from a yellow to a reddish brown, and the latter from white to yellow, by the application of a gentle heat. Periodate of soda gives a green precipitate with copper salts, which becomes more intense by heat, and the proto- and per-salts of iron form yellow precipitates. All these precipitates readily dissolve in dilute nitric acid.—*Journ. de Pharmacie*, Oct., 1836.

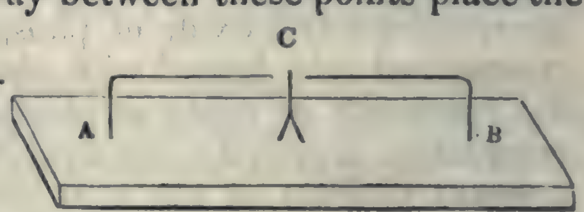
ON AN EXPERIMENT IN ELECTRICITY. BY JAMES WATSON, ESQ.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

As the following experiment may easily be made by any person who has an electrical machine, I think it will be acceptable to many of your readers.

Take a slip of card about one inch in length and one quarter of an inch in breadth, and at one end of the card make a slit up the middle one quarter of an inch long; then bend out the divided parts in opposite directions, so that the bit of card may be made to stand upright upon its two short legs. By this means the card may be so nicely adjusted that a very slight touch will overbalance it, and cause it to fall. Now take two pieces of stout brass wire, four inches in length and pointed at each end; bend the wires at right angles, in order that each wire may have a short arm or stem, one inch long. These short arms or stems are to be inserted a little way into two holes made to receive them in a flat thick piece of wood. The two holes must be made at such a distance from each other that the points of the two long horizontal arms shall be just three-quarters of an inch apart. Midway between these points place the bit of card, in an upright position, as in the figure, where C represents the edge of the card, having its two flat sides opposite to the points of the two wires, A and B.



To insure success in making this delicate experiment, the machine must be screwed to a very *steady* table, otherwise the card will be disturbed by the turning of the cylinder. The best way of making the discharge is to suspend a small jar from the prime conductor, and let the jar discharge itself through the electrometer. A chain must connect the electrometer with the wire A, and another chain must connect the outer coating of the jar with the wire B.

When the experiment is well performed, I always find that the card is perforated, and has a bur on each side of it, but what de-

serves particular notice is the very curious fact that the card is *not thrown down*.

If two bits of card be placed between the wires, instead of one as in the last experiment, even then the separate bits of card will continue *to stand*, although both will be perforated.

The motion of a single fluid from the positive to the negative wire, cannot, I think, be reconciled with my experiment, which seems to require *two equal* repulsive actions.

I am, Gentlemen, your obedient servant,

JAMES WATSON.

London, Jan. 31, 1837.

VOLUNTARY SOUNDS OF INSECTS.

We have received the following communication from a Correspondent relative to Dr. Burmeister's paper "On the Cause of Sound produced by Insects in Flying," printed in the Third Part of the SCIENTIFIC MEMOIRS; in which it is shown not to be caused by the vibration of the wings, but by peculiar organs placed in the thorax, a minute account and delineation of which are given.

"I have several times tried the following experiment on the common large blue fly (*Musca vomitoria*). While it has been flying about the room with its usual buzzing noise, I have placed a small piece of meat on a table, and opened the door of the room. The fly seems to be soon attracted by the meat, and on approaching and hovering over it the buzz has appeared to increase in loudness; but if, when it has been thus hovering over the table, I have caught at it with my hand, or attempted to strike it with a handkerchief, it has immediately flown away, and generally out at the door, without producing the slightest audible sound, as though it intended to conceal the direction of its flight." X.

[I have often witnessed the silent flight of this insect on being disturbed or attempted to be caught, exactly as described by our Correspondent.—E. W. B.]

METEOROLOGICAL OBSERVATIONS FOR FEBRUARY 1837.

Chiswick.—Feb. 1. Foggy: slight frost. 2. Foggy: fine. 3. Hazy. 4. Frosty: fine. 5. Overcast. 6, 7. Sharp frost: very fine. 8. Overcast: rain. 9. Fine. 10. Fine: rain: stormy at night. 11. Boisterous, with rain: lightning at night. 12. Clear: cloudy and fine: rain. 13. Rain: fine. 14. Very fine. 15. Foggy. 16, 17. Fine. 18. Slight rain: cloudy: stormy: about ½ past 10 P.M. a reddish luminous arch was observed, extending through the zenith, in a direction nearly east and west. 19. Overcast: stormy with heavy rain. 20. Very clear: fine. 21. Stormy with rain: fine. 22. Clear and fine. 23. Stormy with rain. 24. Clear and cold. 25, 26. Cold and bleak. 27, 28. Overcast.

Boston.—Feb. 1. Foggy. 2. Cloudy. 3, 4. Fine. 5—7. Cloudy. 8. Fine. 9. Cloudy. 10. Stormy. 11. Rain and stormy: rain early A.M. 12—14. Fine. 15. Cloudy. 16, 17. Fine. 18. Cloudy: stormy with rain P.M. 19. Cloudy: rain P.M. 20. Cloudy. 21. Fine: rain early A.M.: rain P.M. 22. Cloudy. 23. Rain: stormy night. 24. Stormy. 25. Fine: snow P.M. 26. Fine. 27. Cloudy. 28. Fine.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary; by Mr. THOMPSON at the Gardens of the Horticultural Society at Chiswick, near London; and by Mr. VEALL at Boston.

Days of Month. 1837. Feb.	Barometer.				Thermometer.						Wind.			Rain.			Dew-point.
	London: Roy. Soc. 9 A.M.	Chiswick. Max.	Chiswick. Min.	Boston. 8½ A.M.	London: Roy. Soc. Fahr. 9 A.M.	London: Roy. Soc. Self-registering. Min.	London: Roy. Soc. Self-registering. Max.	Chiswick. Max.	Chiswick. Min.	Boston. 8½ A.M.	London: Roy. Soc. 9 A.M.	Chisw. 1 P.M.	Bost.	London: Roy. Soc. 9 A.M.	Chisw.	Boston.	Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
W. 1.	30·018	30·206	30·033	29·65	40·9	40·4	46·3	46	40	40	SE.	E.	calm	·036	...	...	36
Th. 2.	30·292	30·319	30·299	29·90	41·5	41·4	44·3	47	39	39	SSW.	SE.	calm	...	...	...	36
F. 3.	30·348	30·373	30·349	29·97	41·4	40·6	45·3	48	28	40	ENE.	SE.	calm	...	...	...	37
S. 4.	30·386	30·399	30·391	30·	34·8	34·4	45·6	41	33	38	E.	S.	calm	...	...	...	31
● ⊙ 5.	30·372	30·377	30·363	30·	36·5	34·8	39·6	39	26	34·5	ESE.	SE.	SE.	...	...	...	33
M. 6.	30·386	30·487	30·345	30·	33·7	32·3	38·4	43	27	32	E.	S.	calm	...	...	...	28
T. 7.	30·308	30·343	30·298	29·92	33·9	30·0	40·3	46	27	33	E.	S.	calm	...	...	...	31
W. 8.	30·168	30·205	30·192	29·83	38·3	34·2	41·0	46	41	34·5	S.	S.	calm	...	·09	...	32
Th. 9.	30·276	30·295	30·197	29·84	45·3	38·4	46·2	49	44	43	SSW.	SW.	calm	·088	...	...	37
F. 10.	29·934	29·963	29·730	29·40	46·8	45·0	48·7	52	45	46	NE. var.	S.	NW.	...	·20	...	41
S. 11.	29·388	29·404	29·214	28·93	49·0	46·4	51·0	50	37	48	SE. var.	S.	NW.	·166	·34	·22	44
☽ ⊙ 12.	29·480	29·515	29·203	28·97	39·3	37·4	50·0	49	39	40	SSW.	SW.	calm	·175	·16	·18	38
M. 13.	29·292	29·311	29·276	28·75	47·9	39·3	48·8	51	33	45	S.	SW.	calm	·125	·08	·04	41
T. 14.	29·372	29·750	29·384	28·94	40·8	39·7	50·9	52	27	38	SSW.	W.	calm	...	·02	...	38
W. 15.	30·022	30·086	30·049	29·53	38·4	35·6	47·7	52	36	36	SW.	SW.	calm	·033	...	...	36
Th. 16.	30·130	30·159	30·101	29·55	47·8	38·7	48·7	55	40	49	S.	SW.	calm	...	...	...	42
F. 17.	30·278	30·335	30·275	29·65	45·2	44·3	54·0	55	31	45·5	SSW.	W.	calm	...	·02	...	44
S. 18.	30·026	30·055	29·665	29·60	45·8	40·0	52·0	51	32	39	SSE. var.	SW.	calm	...	·19	...	41
⊙ 19.	29·736	29·762	29·162	29·24	40·7	37·0	50·3	50	39	37	S.	S.	calm	·125	·64	·10	37
○ M. 20.	29·508	29·726	29·495	29·00	42·0	40·3	50·6	49	41	40	W.	W.	NW.	·591	·06	·36	39
T. 21.	29·540	29·713	29·550	28·97	46·3	41·8	47·8	54	37	45	SW.	SW.	NW.	·036	·09	·16	42
W. 22.	29·874	30·005	29·892	29·35	41·3	39·2	51·6	50	37	40	SW.	W.	NW.	·094	·04	·03	39
Th. 23.	29·628	29·722	29·323	29·10	44·8	40·4	48·2	51	33	40	WNW. var.	SW.	W.	...	·08	·23	41
F. 24.	29·744	30·019	29·763	29·26	40·3	36·0	51·2	45	32	38	WSW.	NW.	N.	·111	...	·15	35
S. 25.	30·104	30·219	30·134	29·67	35·7	34·0	44·2	42	28	34	WNW.	N.	N.	...	...	...	33
⊙ 26.	30·206	30·260	30·201	29·77	34·4	31·9	40·7	41	30	34·5	WNW.	N.	calm	...	...	...	30
M. 27.	30·010	30·048	29·980	29·60	35·4	33·9	40·2	41	32	36	W.	S.	W.	...	...	...	32
☾ T. 28.	30·078	30·288	30·096	29·70	37·5	35·2	39·4	42	29	38	N.	NE.	E.	...	...	...	33
	29·961	30·487	29·162	29·50	40·9	38·0	46·5	55	27	39·3				Sum 1·580	2·01	1·47	36·7

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LXIII. *Cyanide of Potassium, an incidental Product of the Process for making Cast Iron in Blast Furnaces. By* THOMAS CLARK, *M.D., Professor of Chemistry in Marischal College and University, Aberdeen.**

DURING the last three years, a salt, which, when melted, is clear and colourless, but which, when solid, is of an opake white, and generally not crystalline, has been observed to exude in the liquid state, from cracks, and other accidental outlets, around the tweers of the hot-blast furnaces for making cast-iron, in the Clyde iron-works. The salt occurs in greater quantity at one time than at another; but I have not yet been able to ascertain the circumstance in the process upon which the supply of the salt depends. The workmen say, that it occurs most after what they call *a scour* of the furnace, that is, after, by an excess of the fluxing ingredients of the smelting process, or an increase of the fuel, the materials accidentally adhering to the inner sides of the furnace have been dissolved away. The salt in question, however, may exude, under such circumstances, not because it is produced in greater quantity, but merely because it then finds a readier outlet at the tweer, where alone it has hitherto been observed. At the Clyde iron-works, the salt has occasionally accumulated in such quantity as to require a wheelbarrow for its removal. Upon minute inquiry I found that a similar product, although it had attracted no attention, occurred at other iron-works in Scotland, wherein, as at the Clyde iron-works, the hot blast and

* Communicated by the Author.

coal for fuel were employed; but owing to the rare use of coke in Scotland now, I am, as yet, not aware whether the same product has been observed where coke is the fuel consumed.

The principal ingredient in the salt thus obtained, is *cyanide of potassium.* In a quantity that was, about a year ago, submitted to an examination whereof the sequel will give an account, the cyanide of potassium made up about 53 parts in the 100, the rest being carbonate of potash, intermixed with a small quantity of carbonate of soda; but another quantity that was examined about a year previously,contained more than two thirds of cyanide of potassium. One learns, not without surprise, that so remarkable a product should occur from such materials, and under such circumstances. That potassium should be there, from what source as yet I know not, will indicate that the presence of that element, in even unpromising materials of soils for vegetation, is more general than is usually suspected. Nor will the iron-master fail, from this intimation, to warn all under his charge whom it may concern, of the perilously poisonous character of this product—a warning not idle, I presume, since on a visit to the Clyde iron-works I learned that the workmen, having discovered its alkaline properties, some of their wives, "on household thoughts intent," had resolved to make the cyanide of potassium available in their washing-tubs. The product will, however, better merit the attention of the pharmacian, as affording a copious and cheap source whence to obtain cyanide of potassium.

The details of the investigation, having no novelty of method to boast of, might not be worth giving, were there not strong reasons for believing that, for want of due precautions, a similar product has been more than once—in the hands too of able chemists, mistaken for carbonate of potash. This induces me to give the details.

A portion of the salt, selected as free from insoluble admixture, was dissolved in water, which was done easily, and to the solution, which was distinctly alkaline, dilute nitric acid was added, until the solution, being gently heated, became neutral. Effervescence took place. The evolved gases betrayed the presence of carbonic acid by precipitating lime-water, and of hydrocyanic acid by the smell of it, which prevailed. The neutral solution gave no precipitate by nitrate of barytes, or nitrate of silver,—indicating the absence not only of sulphates and chlorides, but that of salts of several other acids such as these reagents would precipitate. The same solution was unaffected by sulphuretted hydrogen, by sulphuret of potassium, by yellow ferro-prussiate of potash, by oxalate

of ammonia, or by carbonate of potash—showing the absence of all metallic bases, except such as are alkaline.

Potassium was proved to be present, and sodium too, by crystallizing a portion of the same solution, examining the successive crops of crystals that were formed, and persevering in the crystallization until there did not remain a drop of the solution. At first the well-known crystals of nitre were obtained. Towards the end, however, when the solution had become small enough to be transferred to a watch-glass, little crystals of nitrate of soda, in their well-known rhomboidal form, appeared. This test for the presence of sodium is much more delicate than might be imagined. But another and a readier test showed the presence of sodium as decisively. A platinum wire, scrupulously clean, is, by way of precaution, placed either before the tip of the inner blue flame of the blowpipe, or so as to touch the circumference of the blue flame of alcohol. In either case, the colour of the flame is unaffected by the wire, if sufficiently clean. Dipped, however, into a strong solution of any salt containing potassium, and dried above a flame, the wire will, before the blowpipe, show a violet flame, beyond the wire, in continuation of the blue one, but short and spread; and, in the flame of alcohol, it will tint with a like violet colour all above the wire. But a similar wire, dipped into a rather strong solution of a salt containing sodium, and treated in like manner, will give, before the blowpipe, an intense greenish-yellow light, shaped so as to seem a prolongation of the blowpipe's blue flame; and, when placed in the flame of alcohol, will imbue so much of the flame as is above the wire with a similar colour. A sodium salt, although intermixed with one of potassium in a smaller proportion than 1 to 100, will give, with sufficient distinctness, a like indication of its presence. Any common gas-light, lowered till it burns blue, will answer for the detection of sodium in this manner; but, to exhibit this test on the lecture-table, the flame of alcohol answers best. By this test, applied in all these modes, the salt exuded from the blast-furnaces gave distinct indications of the presence of sodium.

The proportion of nitrate of soda obtained by crystallizing the mixed nitrates of the two alkalis was manifestly small; but, as the two were evidently in a state of mixture—not of combination—it did not seem worth while to ascertain the proportions of each. Nevertheless, I made an approximative experiment, in order to form some idea of the relative proportion of the intermixed sodium salt. Equal weights of pure chloride of potassium and of mixed chlorides, formed by treating the salt under examination by pure muriatic acid, were sepa-

rately precipitated by nitrate of silver, the experiment in each case being conducted in a like manner. The chloride of silver from the mixed chlorides, was to the chloride of silver from the chloride of potassium, in the proportion of not more than 1004 to 1000. This corresponds to about 15 of sodium salt in 1000 of the mixture.

That the salt under examination contained no ferro-prussiate of potash, was proved by *first* supersaturating a watery solution of it with pure muriatic acid, *and then* adding a solution of a protosalt of iron. No blue appeared. That the salt did contain cyanide of potassium, was proved by *first* adding, to a watery solution of it, a solution of protosulphate of iron, *and then* redissolving the precipitate by pure muriatic acid; upon which prussian blue appeared.

For want of attending to the *order* in which these reagents should be used, an eminent chemist, expressly seeking for the production of an alkaline cyanide, has merely proved the absence of a ferro-prussiate, where he conceives he proved the absence of an alkaline cyanide.—(*Ann. de Chim. et de Phys.* tom. lix. pp. 26[illegible], 269.)

To ascertain the proportion of cyanide of potassium, the method was adopted—which repeated experience has taught me to regard as the best—of estimating that cyanide from what peroxide of mercury it can render soluble. All the precaution required is, that the peroxide be pure and in fine powder. Accordingly, 12 grains of the salt were dissolved in about 1000 grains of water, and treated with peroxide. Were those 12 grains entirely cyanide of potassium, they would dissolve 20 grains of the peroxide of mercury. In point of fact, they dissolved, in three experiments, as under:

10·77	grains, corresponding to cyanide of potassium..	53·9 in 100
10·77	————————	53·8
10 5	————————	52·5
	Cyanide of potassium	53·4 in 100

Having found, by preliminary experiments, that a given weight of carbonate of potash precipitated from a solution of chloride of calcium the same weight of carbonate of lime, whether pure cyanide of potassium was added to it or not, I resolved to estimate the carbonate of potash by that method. 50 grains of the salt gave, of carbonate of lime, in two experiments:

16·5	grains, corresponding to carbonate of potash	45·3 in 100
16·9	————————	46·3
	Carbonate of potash	45·8 in 100

Together, the result is, for 100 parts,

Cyanide of potassium	53·4
Carbonate of potash	45·8
	99·2
Loss...............	·8

—a result confirmatory of sodium being present in small quantity.

That the salt contained no caustic alkali was thus proved. Into a weak solution of the salt, a solution of nitrate of silver, likewise weak, was dropped. The precipitate was *white*, as would occur from a solution either of cyanide of potassium or of carbonate of potash. Into a similar solution of the salt, one or two drops of a weak solution of caustic potash were let fall, and, *afterwards*, one drop of the nitrate of silver solution; whereupon the *light grayish brown*, indicative of the precipitation of oxide of silver, at once appeared. The absence of this light grayish brown in the previous experiment demonstrated the absence of caustic potash or caustic soda in the salt under examination*.

Marischal College, March 13, 1837.

LXIV. *Further Experiments on the Solubility of certain Metallic Oxides and Salts in Muriate and Nitrate of Ammonia. By* R. H. Brett, *Esq., F.L.S.*

SINCE publishing my paper in the February Number of the Philosophical Magazine, p. 95, I have made further experiments on the same subject, the results of which I now offer for insertion at this time, more especially since a recent criticism has appeared, p. 178, not at all invalidating the accuracy of the experiments, but complaining of what the author terms a "serious omission" likely to produce an erroneous opinion as to the cause by which certain metallic oxides and salts are brought into solution under certain circumstances. My object, in the paper to which I have alluded, was to state certain facts, which I believed to be of considerable importance in chemical analysis, and an ignorance of which must lead to serious errors, especially in certain quantitative investigations. Your correspondent disapproves of the term *soluble in muriate or nitrate of ammonia*, and had I expressly stated that such solution takes place in every instance, without decomposition of the ammoniacal salt employed, I should doubtless have committed a grave error; such, however, was never my belief, and the exact nature of the changes which took place, was

* In reference to the subject of Dr. Clark's paper we may cite a notice in Phil. Mag., First Series, vol. lxii. p. 234.—Edit.

not the subject of my inquiry; to find fault with the expression is therefore, I think, rather hypercritical, as it would be in the common expression that certain metallic bodies, such as zinc or silver, are soluble in nitric acid; for any one using such a term in reference to those substances would doubtless know that before such solution could take place, the acid must undergo decomposition. The experiments which I performed, as well as a repetition of those referred to in my last paper as instituted by M. Vogel, and afterwards by Mr. Smith, could not but have informed me of the fact that the ammoniacal salt does under certain circumstances, if not in all cases, suffer decomposition.

I shall now briefly mention the results of my late experiments on this subject. The oxides and salts operated upon were well washed on filters with distilled water, and allowed to dry at the temperature of the room.

BaryticSalts.—The carbonate and phosphate of baryta, when digested in cold muriate of ammonia in solution (a saturated solution), yield a fluid which, after filtration, contains the earth in some form of combination, it being readily precipitated by dilute sulphuric acid: ammonia in excess causes no precipitate.

Strontian Salts.—Precisely the same results were obtained.

Lime Salts.—The neutral phosphate of lime which possesses a semi-crystalline appearance, and was obtained by adding a neutral solution of phosphate of ammonia to a solution of chloride of calcium in excess, as also the subphosphate, altogether devoid of crystalline structure, and obtained by adding an excess of the salt of lime to the phosphatic salt, containing, according to Berzelius, 1 and $\frac{1}{3}$ more lime than the neutral salt*, when digested in the cold with the saturated solution of muriate of ammonia, yielded by filtration a fluid which was abundantly precipitated by oxalate of ammonia, and therefore contained lime in some form of combination. It may be observed that ammonia added in excess to the filtered fluid obtained by digesting any of the above salts, barytic, strontian, or calcareous, did not cause any precipitate.

Magnesian Salts.—The carbonate and ammoniaco-magnesian phosphate are similarly acted upon.

Oxide and Salts of Cadmium.—The oxide, carbonate, phosphate, and oxalate of cadmium, when digested in a cold saturated solution of muriate of ammonia, yielded a fluid by filtration, abundantly precipitated of a yellow colour by hydrosulphuret of ammonia.

Salts of Cobalt.—The carbonate and phosphate when di-

* Berzelius, *Traité de Chimie*, tom. iv. p. 71 *et seq.*

gested in a cold saturated solution of muriate of ammonia yielded, by filtration, a fluid abundantly precipitated of a black colour by hydro-sulphuret of ammonia.

Oxide and Salts of Manganese.—The brown coloured hydrated deutoxide of manganese when digested in the ammoniacal salt yielded, by filtration, a fluid which was precipitated by hydrosulphuret of ammonia, of the peculiar pale flesh-colour characteristic of salts of manganese; it is not improbable, however, that the oxide operated upon, although apparently of a uniform brown colour, might have contained a little protoxide which had not passed into the state of deutoxide by aerial exposure. Phosphate of manganese when treated in the same manner yielded, by filtration, a fluid which was similarly acted upon by hydrosulphuret of ammonia.

Oxide and Salts of Copper.—The hydrated peroxide of copper, the black anhydrous peroxide, and the carbonate of copper, when digested in cold muriate of ammonia yield, by filtration, a blue coloured fluid, evidently containing the metallic oxide in some form of combination.

Salts of Bismuth.—The subnitrate of bismuth of the shops, when digested in a cold solution of muriate of ammonia or boiled in the same salt did not yield, by filtration, a fluid which yielded a black precipitate by hydrosulphuret of ammonia; the same may be said of the salt when recently precipitated, well washed and dried at the prevailing atmospheric temperature.

The same applies to the protoxide and certain protosalts of *tin.*

Salts of Silver.—The chloride, carbonate, and phosphate when prepared without exposure to light, and digested in the ammoniacal salt, without exposure to light also, yielded, by filtration, a fluid which was precipitated black by hydrosulphuret of ammonia.

Persalts and Oxide of Mercury.—The peroxide, carbonate, phosphate and biniodide, when digested in a cold solution of muriate of ammonia yield a fluid, which is abundantly precipitated of a black colour by hydrosulphuret of ammonia.

Some other metallic oxides and salts still remain to be operated upon; but as circumstances prevent me from carrying on the inquiry at the present moment, I must defer the consideration of them to a future period.

March 2, 1837. R. H. Brett, F.L.S.

N.B. It will be observed that the results arrived at with the salts of bismuth are different from those mentioned in the former paper, p. 98; but it must be remembered that they were operated upon under different circumstances as then noticed.

LXV. *On the Laws of Transmission of Light and Heat in uncrystallized Media.* By P. Kelland, *B.A., Fellow and Tutor of Queen's College, Cambridge, and Member of the Cambridge Philosophical Society.**

THE difficulties under which a mechanical theory labours must necessarily be owing, in the outset, to a view of the subject not sufficiently simple. In the memoir † from which the following pages are extracted, I have endeavoured to reduce the equations of motion of a series of particles transmitting vibrations, to the most simple possible form; and from that form to deduce, conversely, conclusions with respect to the action of the particles themselves. Whether indeed a more general form of the *solution* of the resulting equations may not be the correct one, is another question, which I have discussed in a memoir lately presented to the Cambridge Philosophical Society. It would be out of place again to give a historical view of the subject, as it has been ably done by Professor Powell, in his "Abstract of M. Cauchy's Views‡." I have satisfied myself with developing my views as briefly as I can.

In order to reduce the investigation to the simplest possible form, I will suppose the particles symmetrically arranged with respect to the three coordinate axes, and conceive the transmission to take place along one of these axes, that of x. It is clearly requisite that such an hypothesis should be made when we treat of media not crystallized.

Let x, y, z be the coordinates of the particles P under consideration: when at rest

$x+\delta x$, $y+\delta y$, $z+\delta z$ those of Q.

r the distance P Q,

and at the end of the time t, let

$x+\alpha$, $y+\beta$, $z+\gamma$ be the coordinates of P,

$x+\alpha+\delta x+\delta \alpha$, $y+\beta+\delta y+\delta \beta$, $z+\gamma+\delta z+\delta \gamma$ those of Q,

$r+\xi$ the distance P Q.

Also let $(r\, \phi(x))$ represent the attraction at the distance r.

Then denoting by the symbol Σ the sum of all functions similar to that which follows that symbol, we have

$$\frac{d^2 \alpha}{d t^2} = \Sigma\, \phi\,(r+\xi)\,(\delta x+\delta \alpha).$$

* Communicated by the Author.

† Transactions of the Camb. Phil. Society, vol. vi. part 1.

‡ This abstract will be found in Lond. and Edinb. Phil. Mag. vol. vi. p. 16 *et seq.*—Edit.

By expansion we obtain

$$\phi(r+\varrho)=\phi(r)+\frac{d\,\phi\,r}{d\,r}\,\varrho+\dots\dots\dots$$

and $\quad (r+\varrho)^2=(\delta x+\delta\alpha)^2+(\delta y+\delta\beta)^2+(\delta z+\delta\gamma)^2$

$$=r^2+2\,(\delta x\,\delta\alpha+\delta y\,\delta\beta+\delta z\,\delta\gamma)$$

$\therefore\ \varrho=\frac{1}{r}\,(\delta x\,\delta\alpha+\delta y\,\delta\beta+\delta z\,\delta\gamma)$ omitting small quantities;

and by substituting this value in the above equation, it gives

$$\frac{d^2\alpha}{d\,t^2}=\Sigma\left\{\phi\,r+\frac{d\,\phi\,r}{r\,d\,r}\,(\delta x\,\delta\alpha+\delta y\,\delta\beta+\delta z\,\delta\gamma)\right\}(\delta x+\delta\alpha)$$

but $\Sigma\,\phi\,r\,.\,\delta x$ is evidently the accelerating force, resolved parallel to x, on the particle P in its state of rest, and consequently is equal to zero: we have then

$$\frac{d^2\alpha}{d\,t^2}=\Sigma\left\{\phi\,r\,.\,\delta\alpha+\frac{d\,\phi\,r}{r\,d\,r}\,(\delta x^2\,\delta\alpha+\delta x\,\delta y\,\delta\beta+\delta x\,\delta z\,\delta\gamma)\right\}$$

which we will call equation (1.).

I shall pass over the argument for the *form* of the solution, and assume at once that

$$\alpha=a\cos\,(n\,t-k\,x)$$
$$\beta=b\cos\,(n'\,t-k\,x)$$
$$\gamma=c\cos\,(n''\,t-k\,x)$$

the quantity k being the same for all, as it is the ratio $\frac{2\,\pi}{\lambda}$; λ being the length of a wave. We shall thus obtain,

$$\delta\alpha=a\cos\,(n\,t-k\,x-k\,\delta x)-a\cos\,\overline{n\,t-k\,x}$$

$$=a\cos\,(n\,t-k\,x)\cos k\,\delta x+a\sin\,\overline{n\,t-k\,x}\,\sin k\,\delta x$$
$$-\,a\cos\,(n\,t-k\,x)$$

$$=-\,2\,a\cos\,(n\,t-k\,x)\,\sin^2\frac{k\,\delta x}{2}+a\sin\,(n\,t-k\,x)\sin k\,\delta x$$

$$=-\,2\,\alpha\sin^2\frac{k\,\delta x}{2}+a\sin\,(n\,t-k\,x)\,\sin k\,\delta x$$

$$\delta\beta=-\,2\,\beta\,\sin^2\frac{k\,\delta x}{2}+b\sin\,(n'\,t-k\,x)\,\sin k\,\delta x$$

$$\delta\gamma=-\,2\,\gamma\,\sin^2\frac{k\,\delta x}{2}+c\,\sin\,(n''\,t-k\,x)\,\sin k\,\delta x.$$

Now we have supposed the medium to be one of symmetry: to fix the ideas, conceive the particles arranged in a cubical form, the edges of the cube being parallel to the coordinate axes. Hence for every value of δx there is a set of *pairs*

of values of δy equal in magnitude, and opposite in sign: the same is true of values of δ: the *sum* of all such expressions is consequently equal to zero.

Making all the reductions, the equation (1.) is finally reduced to

$$\frac{d^2 \alpha}{d t^2} = -2 \alpha \Sigma \left\{ \phi r + \frac{d \phi r}{r d r} \delta x^2 \right\} \sin^2 \frac{k \delta x}{2},$$

and by exactly the same process the following equations result for the other directions:

$$\frac{d^2 \beta}{d t^2} = -2 \beta \Sigma \left\{ \phi r + \frac{d \phi r}{r d r} \delta y^2 \right\} \sin^2 \frac{k \delta x}{2}$$

$$\frac{d^2 \gamma}{d t^2} = -2 \gamma \Sigma \left\{ \phi (r) + \frac{d . \phi r}{r . d r} \delta z^2 \right\} \sin^2 \frac{k \delta x}{2}.$$

These equations it must be observed have resulted from the hypothesis that their solution has the form

$$\alpha = a \cos (n t - k x)$$
&c.
$$\frac{d^2 \alpha}{d t^2} = - a n^2 \cos (n t - k x)$$
$$= - n^2 \alpha;$$

hence $$n^2 = 2 \Sigma \left\{ \phi r + \frac{d \phi r}{r d r} \delta x^2 \right\} \sin^2 \frac{k \delta x}{2},$$

&c. &c.

and if v, v', v'', be the velocities of transmission of disturbances, parallel respectively to x, y and z, we evidently have

$$v^2 = 2 \Sigma \left(\phi r + \frac{d \phi r}{d r} \delta x^2 \right) \left(\frac{\sin \frac{k \delta x}{2}}{k} \right)^2$$

$$v'^2 = 2 \Sigma \left(\phi r + \frac{d \phi r}{r d r} \delta y^2 \right) \left(\frac{\sin \frac{k \delta x}{2}}{k} \right)^2$$

$$v''^2 = 2 \Sigma \left(\phi r + \frac{d \phi r}{r d r} \delta z^2 \right) \left(\frac{\sin \frac{k \delta x}{2}}{k} \right)^2$$

I forbear to mention in this place, the facilities which are afforded by these equations to the explanation of dispersion, my object being the especial one of pointing out the reasoning by which we are led to the assumption of the *inverse square of the distance* as one law of force.

It will be evident to any one who attentively studies the above equations, that the expression for the square of the velocity of transmission is a series of terms beginning from

zero, and *increasing* from that value until δx becomes equal to $\frac{\pi}{k}$ or $\frac{\lambda}{2}$, then diminishing till δx becomes λ, afterwards increasing and diminishing in the same manner.

And further, if the force vary according to an inverse power of the distance, or to a function which can be expanded in inverse powers, the terms introduced by the successive half-waves will diminish rapidly; the whole value of the square of the velocity will then depend essentially on that term which depends on the first half-wave.

For this term let us expand the sine and omit the other terms; or rather suppose this one to represent truly the *form* 7, and we obtain the following expression:

$$v^2 = \frac{1}{2}\,\Sigma\left\{\phi r + \frac{d\,\phi\,r}{r\,d\,r}\,\delta x^2\right\}\left\{\delta x^2 - \frac{\pi^2}{3\,\lambda^2}\,\delta x^4 \ldots\right\}$$

and similar expressions for v'^2 and v''^2.

Now suppose $\phi\,(r) = \frac{1}{r^{n+1}}$, or that the force varies as the inverse nth power of the distance: then $\frac{d\,\phi\,r}{r\,d\,r} = -\frac{u+1}{r^{n+3}}$ and if ε be the distance between two consecutive particles, and

$$\delta x = \xi\,\varepsilon, \quad \delta y = \eta\,\varepsilon, \; \delta z = \zeta\,\varepsilon$$

we shall have

$$v^2 = \frac{1}{2}\,\Sigma\left\{\frac{1}{\varepsilon^{n+1}\,(\xi^2+\eta^2+\zeta^2)^{\frac{n+1}{2}}} - \frac{\overline{n+1}\;\xi^2}{(\xi^2+\eta^2+\zeta^2)^{\frac{n+1}{2}}}\right\}\left\{\xi^2\,\varepsilon^2 - \frac{\pi^2}{3\,\lambda^2}\,\zeta^4\,\varepsilon^4\right\}$$

$$= \frac{1}{2\,\varepsilon^{n-1}}\,\Sigma\left\{\frac{1}{(\xi^2+\eta^2+\zeta^2)^{\frac{n+1}{2}}} - \frac{(n+1)\,\xi^2}{(\xi^2+\eta^2+\zeta^2)^{\frac{n+3}{2}}}\right\} \times \left\{\xi^2 - \frac{\pi^2}{3\,\lambda^2}\,\frac{\xi^4\,\varepsilon^2}{\lambda^2}\right\}$$

$$= \frac{p}{\varepsilon^{n-1}} - \frac{q}{\varepsilon^{n-3}}\;\frac{1}{\lambda^2}.$$

As we are not able to integrate or sum the above series directly, we are obliged to make some hypothesis respecting it; the most obvious appears to be that which we have adopted, p and q being numerical factors.

The velocity with which light is transmitted is greater *in vacuo* than in refracting media, whilst no perceptible *variation* of velocity is occasioned by the different lengths of waves.

The same circumstance, therefore, which *increases* $\frac{p}{\varepsilon^{n-1}}$ diminishes $\frac{q}{\varepsilon^{n-3}}$.

Now if $\overline{n-1}$ and $\overline{n-3}$ had both the same sign, an increase in ε would increase or diminish *both* the above quantities at the *same time*; and a like effect would result from a diminution of ε.

We must therefore have one positive and the other negative; which can only be effected by supposing n to be either a fraction, or equal to 2: the latter supposition is the more probable, and we shall presently see additional reasons for supposing it the true one.

Again, since the velocity is greatest *in vacuo* where the variation of velocity is least; it follows that ε must be *less* or the density of the other *greater in vacuo* than in refracting media.

By substituting this value of the law of force, we obtain

$$v^2 = \frac{1}{2}\,\Sigma\left(\frac{1}{r^3} - \frac{3\,\delta x^2}{r^5}\right)\ \sin^2\frac{\pi\,\delta x}{\lambda}$$

$$= \frac{1}{2}\,\Sigma\,\frac{\delta x^2 + \delta y^2 + \delta z^2 - 3\,\delta x^2}{r^5}\,\sin^2\frac{\pi\,\delta x}{\lambda}$$

$$= \frac{1}{2}\,\Sigma\,\frac{\delta y^2 + \delta z^2 - 2\,\delta x^2}{r^5}\ \sin^2\frac{\pi\,\delta x}{\lambda}.$$

But $$\Sigma\,\frac{\delta y^2}{r^5}\ \sin^2\frac{\pi\,\delta x}{\lambda} = \Sigma\,\frac{\delta z^2}{r^5}\ \sin^2\frac{\pi\,\delta x}{\lambda},$$

from the symmetry of the medium

$$\therefore\ v^2 = \frac{1}{2}\,\Sigma\,\frac{2\,\delta y^2 - 2\,\delta x^2}{r^5}\ \sin^2\frac{\pi\,\delta x}{\lambda}$$

$$= \Sigma\,\frac{\delta y^2 - \delta x^2}{r^5}\ \sin^2\frac{\pi\,\delta x}{\lambda}$$

$$v'^2 = \frac{1}{2}\ \Sigma\,\frac{\delta x^2 + \delta z^2 - 2\,\delta y^2}{r^5}\ \sin^2\frac{\pi\,\delta x}{\lambda}$$

$$= \frac{1}{2}\,\Sigma\,\frac{\delta x^2 - \delta y^2}{r^5}\ \sin^2\frac{\pi\,\delta x}{\lambda}$$

$$= -\frac{1}{2}\ v^2.$$

Similarly $$v''^2 = -\frac{1}{2}\,v^2,$$

from which it appears that of the three disturbances either one gives rise to a vibration, and two do not, or *vice versâ.*

Let us then examine the circumstances which will determine each of these results respectively.

We have conceived that the action of a particle in advance of the given particle is to draw it forwards, which is in fact supposing the force *attractive.*

Now, $v^2 = \Sigma . \frac{\delta y'^2 - \delta x'^2}{r'^5} \sin^2 \frac{\pi \delta x'}{\lambda}$,

where r', $\delta x'$, $\delta y'$, are *particular* values of the unaccented quantities.

Now, for every value of $\delta y'$ there is a corresponding value of $\delta x'$ equal in magnitude, and the term in the series will therefore be

$$\Sigma \frac{\delta x'^2 - \delta y'^2}{r'^5} \sin^2 \frac{\pi \delta y'}{\lambda},$$

$\delta y'$ being now a particular value of $\delta x'$, and so on.

Hence v^2 consists of the sum of a series of the form

$$\Sigma \left\{ \frac{\delta y'^2 - \delta x'^2}{r'^5} \sin^2 \frac{\pi \delta x'}{\lambda} + \frac{\delta x'^2 - \delta y'^2}{r'^5} \sin^2 \frac{\pi \delta y'}{\lambda} \right\}$$

$$= \Sigma \frac{\delta y'^2 - \delta x'^2}{r'^5} \left(\sin^2 \frac{\pi \delta x'}{\lambda} - \sin^2 \frac{\pi \delta y'}{\lambda} \right).$$

And, restricting ourselves to the same limits as before, if $\delta y'$ be greater than $\delta x'$ $\sin^2 \frac{\pi \delta y'}{\lambda}$ is greater than $\sin^2 \frac{\pi \delta x'}{\lambda}$, and the above expression is clearly *negative.* Also it is perfectly evident, from what we have before observed, that the sign of the whole will be the same as that from the particles within the range of the first half-wave.

Hence v^2 is negative, or a vibration in the direction of transmission impossible: and since $v'^2 = v''^2 = -2 v^2$, the vibrations which result from disturbances perpendicular to the direction of transmission are transmitted with the same velocity whatever be the direction of disturbance.

Hence the vibrations in a medium exerting *attractive* forces, varying according to the *inverse square* of the distance, are necessarily transversal.

In the same manner it may easily be shown that were the force *repulsive,* the particles *must* vibrate in the direction of transmission. Of the last conclusion, that *repulsive* forces give rise to *direct,* and attractive forces to *transverse* vibrations, I have offered a popular explanation as follows:

" A series of repulsive particles constituting any vertical

line being simultaneously impelled in a horizontal direction, would, by virtue of their repulsion, cause a similar motion in those immediately in front of them, whilst the latter particles would tend to check the impetus of the former, and thus vibrations in the direction of transmission are simple to conceive and easy to explain. But suppose the forces attractive. Let the system of particles in a vertical line have a *vertical* motion, and the slightest consideration will show us that the immediate consequence is the production of a vertical motion in the particles immediately in advance of them; whilst, as before, the reciprocal action of the latter particles tends to impede the motion of the former. Here, then, we have as clear a case as before, and our general conclusion from the whole is, that repulsive forces allow of *direct*, attractive of *transversal*, vibrations only."—(Trans. Camb. Phil. Society, vol. vi., Part I., p. 178.)

The three equations of motion will then finally be reduced to the form

$$\frac{d^2\alpha}{dt^2} = + 2c^2.\alpha$$

$$\frac{d^2\beta}{dt^2} = - c^2\beta$$

$$\frac{d^2\gamma}{dt^2} = - c^2\gamma.$$

The first of these I have developed in a paper read in May, seeing reason for its application to the phænomena of heat.

[To be continued.]

LXVI. *On the Physical Causes of the principal Phænomena of Heat.* *By* JOHN BARTON, *Esq.**

IT has always appeared to me that the corpuscular hypothesis is capable, with some modification, of affording a more complete and satisfactory explanation of the phænomena of heat and light than the undulatory hypothesis. On the present occasion I propose, without entering into any controversial discussion, to show in what manner the principal phænomena of heat may be deduced from the action of two forces. An attractive force between the particles of heat and those of solid matter, a repulsive force between the particles of heat themselves.

I assume that the particles of heat are very small in comparison of the particles of solid matter, and that these last are very small in comparison of the intervals by which they are

* Communicated by the Author.

separated from each other. The phænomena also require that the repulsive force should decrease more rapidly than the attractive force. These premises being admitted, the following consequences may, if I do not mistake, be deduced from them. For brevity's sake I omit the demonstrations, presuming that they will be readily supplied by those versed in mathematics.

1. If a particle of heat approach a particle of solid matter, it will either fall to the surface and remain there, or it will describe a curvilinear orbit thereabout. According to the direction and velocity of its approach, this orbit will either be confined within a certain limit, like an ellipse, or will go off to an infinite distance, like an hyperbola. To avoid circumlocution, I will take leave to call these two classes of curves respectively, *ellipsoidal* and *hyperboloidal* curves. A particle of heat reposing on the surface of a particle of solid matter, or revolving about it in an ellipsoidal curve, will have no tendency to fly off or to pass into a contiguous body, unless disturbed by some exterior force. It is therefore *latent*.

It must not be supposed that latent heat exists only in fluid or gaseous bodies. The phænomena of softness and malleability must be considered as resulting from the presence of a portion of heat in this state. Nay, it is probable that even the hardest and most brittle substances are not entirely deprived of it.

2. When a solid body is exposed to friction or percussion, a certain number of the particles of heat which had evolved tranquilly in ellipsoidal curves are forced out of their orbits, and fly off beyond the reach of the attractive force of the particle to which they belonged; just as our earth might be driven out of the limits of the solar system by the stroke of a comet; supposing the latter of sufficient density and magnitude. And thus we have an explanation of the heat produced by friction or percussion; which has been supposed to form one of the strongest objections to the corpuscular hypothesis.

It confirms this theory, that a piece of iron which has been heated by hammering becomes *brittle*; indicating that a portion of its latent heat has been lost, while sensible heat has been disengaged.

3. If we suppose a particle of matter to be of an oblong form, or to have one of its axes greater than the others, the particles of heat will collect chiefly about the middle of its length. And thus we have an explanation of a remarkable fact discovered by M. Mitscherlich, that crystallized bodies, when heated, do not expand equally in all directions*.

* See Phil. Mag., First Series, vol. lxiv. p. 162; and Lond and Edinb. Phil. Mag. vol. i. p. 413.—Edit.

4. When the coefficients of the attractive and repulsive forces bear a certain relation to each other, and to the original velocity of approach, the orbit described will resemble a *conchoid*, its two branches being ultimately in one and the same line. In this case the body is *diathermanous*, the rays of heat passing through it apparently in a straight course. The course of a ray through a solid body can never be in fact straight, unless its velocity be infinite; but in proportion as the velocity approaches this limit, the ray will experience less disturbance in passing through bodies imperfectly diathermanous. And accordingly it is found, that rays from incandescent bodies pass through some media which do not admit the passage of rays issuing from bodies of lower temperature, the velocity of projection evidently depending on the repulsive force; and this, in its turn, on the temperature.

5. It must not however be supposed that opake or *adiathermanous* bodies are impervious to the rays of heat. They differ from transparent or diathermanous bodies only in this, that the entering ray, instead of passing through immediately, and escaping from the opposite side, is entangled in a circuitous and irregular course among the particles, until by chance it reaches the surface, when it escapes, unless the angle of its direction with the tangent be so small that it is drawn back again by the attractive force. A particle of heat is therefore detained much longer in passing through an opake than through a transparent body, and contributes much more to raise its temperature, and hence it also appears why the passage of heat through opake bodies is very much slower than through transparent media.

6. The particles of solid bodies are drawn towards each other by two forces, their mutual attraction, and their attraction for the atmospheres of heat about the others. They are also kept apart by two forces, the mutual repulsion of those atmospheres of heat, and the repulsion of the particles of free or sensible heat, which happen at the time to be passing between them. As long as the number of these last remains unchanged, the balance of the opposing forces is a balance of stable equilibrium, in as much as the attractive force diminishes with the distance in a less ratio than the repulsive force. If the number of particles of sensible heat is increased, the body dilates till the equilibrium is restored; but if this dilatation proceed to a certain point, the orbits of a certain number of the revolving particles of heat will be changed from the *hyperboloidal* to the *ellipsoidal* form; that is to say, a certain quantity of sensible heat will be converted into latent heat. At the same time another portion of sensible heat will be acquired from the surrounding bodies and the united re-

pulsive force of these two portions of heat will overbalance the forces of attraction. Thus we have an explanation of the phænomena of *vaporization.*

An account of the causes of this last class of phænomena somewhat resembling the above has been given by Laplace in the twelfth book of the *Mécanique Céleste.* Laplace however gives no explanation of the difference between latent and sensible heat: he attributes the phænomena of expansion exclusively to sensible heat, but does not explain why heat when it becomes latent should lose its repulsive force: he supposes the particles of heat to form atmospheres about the particles of solid matter, which are at rest unless disturbed; and in order to explain why sensible heat tends continually to fly off, he supposes the particles of matter to be in a state of continual agitation.

7. In the same chapter Laplace has given an explanation of the phænomena of liquefaction, which appears to me to require modification. "Every molecule of solid matter," he says, "is subjected to the action of three forces: 1st, the attraction of the surrounding molecules; 2ndly, the attraction of the caloric of those molecules, *plus* their attraction for its caloric; 3rdly, the repulsion of its caloric by the caloric of those molecules. The first two forces tend to bring the molecules nearer together, the third tends to separate them. The three states of *solidity*, *fluidity*, and *gaseous elasticity* depend on the respective efficacy of those forces. In the state of solidity, the first force is the most powerful; the influence of the figure of the molecules is very considerable, and they are united in the direction of their greatest attraction. The increase of caloric lessens this influence by dilating the body; and when this increase is such that the influence in question is very small or none, the second force predominates, and the body takes the liquid form."

I am unable to comprehend why the attractive force between two solid particles should be suddenly reduced to nothing when they are separated to a certain distance one from the other. And this suggestion appears still less probable when it is considered that many crystallizable bodies cease to contract, and even undergo a degree of expansion, at the moment of congelation. The act of changing from fluid to solid depends then on some other cause than the approximation of the particles. Laplace has, I think, truly assigned the immediate physical cause of solidity, when he says that in solid bodies the particles are placed relatively to each other in the position of greatest attraction; but he has not given an adequate reason for their assuming this position. To

supply this deficiency, it may be useful to revert to an explanation above given of an observation of Mitscherlich[1] on the unequal expansion of crystallized bodies in different directions. It was suggested that the particles of heat do not accumulate equally round every part of a particle of solid matter if its axes are of unequal length. Now, this inequality of distribution is such, by the nature of the case, as to counteract the tendency of the particles to place themselves in the position of greatest attraction. In other words, the compound force exerted by the central solid nucleus, with its surrounding atmosphere of heat, will approach more and more nearly to that of a sphere as the heat increases; and by consequence the figure of the particles exercises a less and less influence on their mutual position. When that influence is completely neutralized, the body is a fluid.

8. As the mutual attraction of the particles is not however destroyed in fluids, these particles still arrange themselves in the position of greatest attraction. But this position is no longer influenced by their figure; it is now such precisely as would be assumed by them if spherical, that is to say, the position in which the distance of their centres is a minimum. Now this is the position in which the sum of the intensities between them is also a minimum; therefore the whole bulk of the fluid just before congelation is a minimum. If we suppose other forces to come into operation, those, for instance, resulting from the figure of the particles, the dimensions of the body will consequently be enlarged; and thus it appears why so many substances are found to expand in the act of congelation.

9. Further, as the position of greatest attraction amongst the particles of a solid is that in which their salient angles are presented towards each other, it follows that at the moment of solidification, when this arrangement is established, the orbits of a certain portion of the accompanying particles of heat will be changed from the *ellipsoidal* to the *hyperboloidal* form; that is to say, a certain quantity of latent heat is converted into sensible heat.

10. Those solid bodies which are composed of the largest, or rather the heaviest particles, are at once the best conductors and the worst radiators of heat; for the velocity increases with the mass. Hence the metals are the best conductors of heat, and as far as the experiments hitherto made enable us to judge, their conducting power seems to follow the same order with the magnitude of their component atoms, the differences not being greater than may be reasonably supposed to arise from errors of observation. For a similar rea-

son a particle of heat attempting to escape from the surface of a metallic body is more strongly drawn back by the attractive force of the particles than at the surface of other bodies. The metals have therefore less radiating power than any other substances.

11. The particles of heat emerging from the surfaces of solid bodies at angles so small as to be drawn back again into their substance, form collectively an atmosphere enveloping those surfaces, and extending, there is reason to believe, to distances considerably greater than the interval which separates the solid particles from one another. It is to this atmosphere that we must attribute the repulsive power exerted at the surfaces of bodies, metallic bodies especially, by virtue of which mercury is depressed in a barometer tube, and a steel needle floats on the surface of water. By strong pressure this enveloping atmosphere may be expelled from between two metallic surfaces, and the attractive force of their particles then coming into play, they adhere with considerable force. The reflexion of heat at the surfaces of bodies also appears to be due to the action of this enveloping atmosphere.

12. It may facilitate the apprehension of my meaning to observe, that a particle of matter, with its revolving particles of heat, is supposed to have a resemblance to the sun, with its accompanying bodies, in the solar system. The planets, revolving in elliptic orbits, represent the particles of latent heat; the comets, if indeed any of them revolve, as formerly supposed, in parabolic or hyperbolic orbits, represent the particles of sensible heat. Future observations will, perhaps, enable us to determine the law of the forces by which these minute movements are regulated, as accurately as we are now acquainted with the law of gravitation. The preceding conclusions are, however, independent of the particular law of those forces.

January 27, 1837.

P.S. I have assumed, in conformity with the views of Laplace and other mathematicians, that the particles of solid matter mutually attract each other. But the preceding conclusions hold good though no such force of attraction exists, or even if we suppose with Æpinus that the particles mutually *repel* each other, a supposition which is by no means incompatible, as Dr. Roget has observed*, with the phænomena of gravitation. There are certainly strong reasons in support of this last hypothesis.

* See Scientific Memoirs, Part III. p. 469.—Edit.

LXVII. *On the Composition and Origin of Porcelain Earth.* *By* HENRY S. BOASE, *M.D., Secretary of the Royal Geological Society of Cornwall, &c.**

HAVING lately seen in the *Annales de Chimie et de Physique* (tom. lxii. p. 225.) an interesting paper on Kaolin by Berthier, the conclusions of which, however, do not seem to me to be perfectly satisfactory, I am induced to offer a few remarks, together with an account of some experiments on the Cornish kaolin, in hopes of calling attention to this curious but complicated problem in "the chemistry of geology."

Berthier commences by observing that in a former analysis he found the kaolin of Limoges to be composed of

Silica	46·8
Alumina	37·3
Potassa	2·5
Water	13·0
	99·6

The presence of the alkali he attributed to a mixture of undecomposed felspar; and this being deducted, he considers that the remaining pure plastic clay is a silicate of alumina, represented by the formula AS + Aq, as also given by Forchammer for the porcelain earth of Bornholm. But by a recent set of experiments Berthier has found a considerable portion of magnesia in kaolin from various localities, as shown by the following analyses of the pure argillaceous parts, previously separated from the intermixed substances, by the successive application of sulphuric acid and of a solution of potassa.

	Limoges.	Pamiers.	Elbogen.	Dept. de l'Allier.
Silica	43·05	45·	61·4	56·
Alumina	40·00	38·	23·2	37·
Magnesia	2·89	1·2	0·5	much
Water	14·06	11·7	13·8	12·3
	100·	95·9	98·9	

These specimens of kaolin, with the exception of the first, do not appear to have been well prepared; indeed, even the clay of Limoges cannot be compared with that of Cornwall, since it contains from 20 to 25 per cent. of an undecomposed mineral; whereas the largest quantity which I have obtained from good Cornish samples is under 10 and as low as 8·5 per cent.

* Communicated by the Author.

The substances which Berthier separated from kaolin are granules of quartz and minute white or yellowish-white scales having a pearly lustre. The composition of the scaly mineral he ascertained to be as follows:

	Limoges.	Pamiers.	By calculation.
Silica	65·9	59·2	67·7
Alumina.........	20·8	25·2	19·1
Potassa	7·5	...	9·8
Soda	...	8·9	
Magnesia	2·8	0·5	3·4
Lime	...	1·9	
Water	1·0	3·2	
	98·	98·9	100·

This mineral Berthier regards as the peculiar felspar from which the kaolin has been derived by decomposition. He has taken some pains to establish this point; and has attempted by calculation, founded on the above analyses, to give the true atomic composition of this supposed felspar. I am, however, inclined to think that he is mistaken: for the unaltered mineral which I have extracted from the Cornish kaolin is decidedly a variety of talc, in an extreme state of comminution, and its appearance under the lens exactly agrees with that described by Berthier. Now this mineral abounds in the talcose granite, or protogine, which by disintegration furnishes the beds of porcelain clay; but it does not experience any change, remaining in the clay, from which it is separated in considerable quantity during the process of preparation for the potteries. It seems to be *nacrite* or *scaly talc* of mineralogists, of which Vauquelin has given an analysis, nearly approximating to that of the scales from Pamiers as above quoted.

In denying that kaolin has been formed from the scaly mineral with which it is intermixed, I am willing to admit that it has been derived from a potasso-magnesian felspar which may be similar in composition to that indicated by Berthier's calculation. Indeed several years ago I suggested, on mineralogical considerations, that the felspar of protogine probably contains magnesia. This conjecture is now confirmed by the detection of this earth in kaolin; and I have since ascertained that it is also present in the china-clay of Cornwall.

The specimens examined were of the best quality from Breage and St. Stephens. The process which I adopted was very similar to that employed by Berthier, and gave the following results.

	Breage.	St. Stephens.
Silica	40·15	39·55
Alumina............	36·20	38·05
Magnesia	1·75	1·45
Water	11·65	12·50
Insoluble residue (quartz and talc)	9·50	8·70
	99·25	100·25

The circumstance most to be noted in all the analyses of kaolin hitherto made, is the great discrepancy in the results, which is unavoidable, because this substance is necessarily heterogeneous, quartz and talc being mixed with the silicate of alumina in variable proportions, even when washed with the greatest care. Berthier's process will show pretty nearly the quantity of silicate in the kaolin; but it includes two sources of error, for some alumina will escape between the scales of the talc, and some quartz will be dissolved by the alkaline solution. If, however, the earthy salt be completely separated, this does not indicate the actual composition of kaolin as an article of commerce, for the talc and quartz are not prejudicial to it; indeed protogine itself, under the name of *china-stone*, or *petuntze*, is ground and mixed with the porcelain-earth at the potteries.

I would also remark that the paper of Berthier, acceptable as it is as an analytical essay on an interesting subject, affords another instance of the great bias which the atomic system gives to reduce experimental numbers according to this Procrustean rule. Thus the kaolin is found to contain potassa; this is acutely referred to the presence of an unaltered mineral, proved by analysis to be the source of the alkali. But then its constituent parts, as obtained by experiment, are respectively drawn out or cut off, in order to suit the calculated composition of a potasso-magnesian felspar; and the evil has not stopt here, for the constituents of the kaolin are altered on the same data; and on these also it is attempted to explain the nature of the process by which kaolin is produced from felspar.

I now proceed to notice the perplexing problem concerning the formation of porcelain-earth.

Some have supposed this clay to be an original production, or rather a friable deposit from which the subjacent crystalline rock is now in progress of reconstruction. It is, however, at the present day generally admitted that it owes its origin to a chemical change effected in various kinds of felspathic rocks; but the precise nature of this change is yet unascertained.

Werner was the first to attribute it to the action of water containing free carbonic acid, by which the alkali of the felspar is gradually abstracted. This opinion prevailed among chemists until Berthier showed that silica is also carried off dissolved in the alkaline solution, leaving a silicate of alumina, the constituents of which are consequently not in the same proportion as in felspar.

The late Dr. Turner has illustrated this subject in a very clear and pleasing manner, (Philosophical Magazine for July 1833,) explaining the change by the following formula:

$$\overset{\text{Felspar.}}{(\dot{Po} + 3\,\dot{Si}) + (\underline{\dddot{Al}} + 9\,\dot{Si})}; \quad \overset{\text{Porcelain-earth.}}{(\underline{\dddot{Al}} + 3\tfrac{1}{2}\,\dot{Si})}.$$

The potassa is first set at liberty by the action of water and carbonic acid, and the composition of the felspar being thus subverted, the silica whilst in a nascent state is dissolved by the alkaline solution with which it is in immediate contact. "The formula," says Dr. Turner, "showed that every two equivalents of alumina present in porcelain clay along with three and a half of silica, corresponded in the original felspar from which it was derived, to twelve equivalents of silica and one of potash. Hence the quantity of silica carried off was enormous."

The process of decomposition from which kaolin results has also been well described by Fournet as quoted by Becquerel (*Traité de l'Electricité et du Magnétisme*, tom. i. p. 503), who has collected much interesting information on this subject.

"'Le feldspath, quand il est désagrégé et terreux,' dit M. Fournet, ... 'absorbe donc l'acide carbonique, qui réagit sur les silicates et s'empare de leur bases les plus fortes. La silice est mise en liberté à un état gélatineux qui lui permet de se dissoudre en certaine quantité, à la verité, dans les eaux et dans les carbonates alcalins; elle est alors entrainée par elles, et donne naissance, suivant les circonstances, à des cristaux de quartz hyalin, des fiorites, des agates, des opales, des concrétions calcédoines, et des silicates de nouvelle formation, telles que les mésotypes, les chabasies, &c.'"

These explanations are consistent with our present knowledge of chemistry; but they rest too much on induction, and not on actual experiment. For, in the first place, before the precise nature of the decomposition can be determined, it is necessary to ascertain the composition of the felspar; and I am not aware that this has ever been done. At all events, the felspar in the foregoing formula (which indeed is the one usually referred to on this subject) is the mineral which enters into common granite and not into protogine. And since these rocks occur passing gradually into each other in alternating

series, not only in Cornwall, but also in the Alps and in the Vosges, and are situated under precisely the same circumstances, the fact that the one is decomposed to a hundred times the extent of the other would alone point at a difference in the composition of the felspar of the respective rocks. This is now, in some measure, established by the foregoing analyses of kaolin. It is, however, desirable that the felspar of the protogine should also be examined, though it will not be easy to procure a proper specimen for this purpose; and the chemist must be cautious in his selection of one, that he does not take it from the micaceous or shorlaceous granites with which the protogine is associated.

We have yet so much to learn concerning the properties of alumina and its combinations, that it is difficult to give a satisfactory account of the changes which occur even during the decomposition of common felspar. It is generally supposed that the alkali is first set at liberty and then acts on the nascent silica; but why not also on the alumina whilst in the same condition, or on the silicate of alumina, and still more on the alkalino-silicate of alumina, which is very soluble in alkalis and is most probably present in disintegrating felspar?

But supposing these difficulties surmounted, we have still the first and fundamental change to account for, viz. the subversion of the powerful affinity by which the constituents of the felspar are united. It is an easy matter to say that it is effected by the long-continued action of water and carbonic acid, but what is the *modus operandi*? In the laboratory, we cannot dissever the component molecules of this mineral by the most powerful acids; how then can the weakest effect it in Nature?

Berzelius has made some excellent observations on this subject, (*Traité de Chimie*, tom. iv. p. 574,) instancing other substances, both natural and artificial, which resist solution in acids, contrary to what might be inferred from the nature of their composition; and he concludes that the elements of compound bodies do, in reality, combine in two distinct states of union.

Fournet, who has paid great attention to the difficult subject under consideration, is of opinion that the felspar must be first disintegrated (*désagrégé*) before the chemical action can commence; and he conceives that this is actually accomplished in consequence of felspar possessing the property of dimorphism, the new or second arrangement of the particles causing the disintegration. Becquerel justly remarks on this solution, that we have yet to learn that the particles of igneous rocks, on consolidation, did not assume a permanent form; and that they have experienced by the lapse of time a change

in their arrangement. It may also be added, that if felspar be dimorphic, how comes it to pass that the granite (abounding in felspar) in which the protogine occurs is buts lightly altered?

It appears more probable that a difference in composition in the felspars of these rocks will afford the true solution of this problem, and though the data on which we can at present reason are very imperfect, yet I am inclined to think that the presence of magnesia in the felspar of protogine may, among other causes, contribute toward the extraordinary change which this rock experiences. Thus, the magnesia may absorb carbonic acid, as well as the alkali, from the percolating water; and so great is its tendency to combine with two proportions of this acid, that even one part of the carbonate will attract the acid of the other, so as to pass into a bicarbonate of magnesia; in which state being soluble in water, it would be speedily removed. This in some measure explains the origin of kaolin, and it also accounts for the small quantity of this earth remaining in the porcelain clay; indeed I have examined some samples in which I could not detect a trace of magnesia.

This subject is one of great importance to the geologist, as affording an insight into a first and elementary step in the mighty changes which the crystalline materials of the globe have undergone; and the prosecution of this subject by chemists would confer a boon on geology, and at the same time cannot fail to be instructive to themselves by leading to a better knowledge of the combinations of alumina.

LXVIII. *On Antimoniuretted Hydrogen, with some Remarks on* Mr. Marsh's *Test for Arsenic. By* L. Thompson, *Esq., Member of the Royal College of Surgeons.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I BEG leave to direct the attention of your readers to a hitherto unnoticed combination of antimony and hydrogen, which acquires great interest from its near resemblance in many respects to arseniuretted hydrogen. The plan which I adopt for procuring this gas in its greatest purity is by fusing together equal weights of antimony and zinc, and acting on the alloy with diluted sulphuric acid; this process is not perhaps altogether free from objection, but answers very well for general purposes. As thus prepared antimoniuretted hydrogen is a colourless inflammable gas, exploding violently by the electric spark or lighted taper when mixed with an equal volume of oxygen, chlorine, or atmospheric air; its odour is

peculiar and approaches nearly to that of arseniuretted hydrogen; inflamed at a jet in the open air it burns with a pale bluish green flame resembling that of arseniuretted hydrogen, and gives off a dense white volatile vapour, which collects, as a semicrystalline oxide, on cold bodies placed over it, affording another instance of the similarity of these gases: when a piece of cold glass or china is held in the flame a metallic crust is deposited, and when a tube of glass is used, the metallic film is formed on that part of the tube nearest the flame, and the white oxide around and above it. It is unnecessary to add, that these appearances coincide, in a very remarkable manner, with those produced by arseniuretted hydrogen under similar circumstances; and although a practised eye may discern some difference between the crusts, that from antimony being more silvery and metallic, yet the line of demarcation is not easily drawn, for a thin film of antimony looks more like arsenic than antimony, and a thick crust of arsenic has the metallic appearance of antimony. When sulphuretted hydrogen is passed over the oxides of these metals, the antimonial oxide will become of a darker yellow than the arsenical; but this is also fallacious, for a small quantity of antimony gives a yellow not darker than orpiment, and if any metallic arsenic be present in the arsenical oxide, a portion of realgar forms and gives the product an orange hue. The ammoniaco-sulphate of copper is liable to similar objections, for a large quantity of the oxide of antimony produces a whitish green precipitate which might easily be mistaken for Scheele's green. The two metals may, however, be distinguished by adding a drop of nitric acid to the crusts; they will immediately dissolve, and on evaporating to dryness a white powder is left in each instance. A few drops of a dilute solution of the nitrate of silver being now added, and the whole exposed to the fumes arising from a stopper moistened with ammonia, the antimonial solution will be observed to deposit a dense white precipitate, whereas that from arsenic will give the well-known Canary-yellow flocculi. I prefer this mode of using silver to the ammoniaco-nitrate of the same metal, for the slightest excess of ammonia destroys the colour, but by watching the effect of the vapour, the exact quantity requisite is easily obtained.

For the purpose of testing, it is unnecessary to use the alloy which I have mentioned, as the gas arising from one grain of tartar emetic, or any other salt or oxide of antimony, with a little diluted sulphuric acid and zinc, will furnish an abundance of metallic crusts; indeed a single drop of the common wine of antimony will produce a very distinct film. In accordance with the names already given to similar compounds of hydrogen and the metals, I have called this gas antimoniuretted hydrogen.

All circumstances considered, I fear we can only regard Mr. Marsh's very ingenious test for arsenic as furnishing good collateral evidence, capable indeed, in scientific hands, of giving very correct indications, but wholly unfit to be entrusted to those unaccustomed to careful chemical manipulation. I say this with a thorough conviction of the great utility of Mr. Marsh's test, and am only sorry that its evidence is not unequivocal.

Roebuck Place, Great Dover Road.

Yours, &c., LEWIS THOMPSON, M.R.C.S.

LXIX. *Notice of a Theory of Molecular Action. By* PAUL COOPER, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the Philosophical Magazine for April, p. 320, you have given a notice of the researches of M. Mossotti relative to the laws of molecular action; may I beg the favour of your directing the attention of your readers to an equally simple and much more comprehensive theory on the same subject, a sketch of which I published in a small pamphlet* some months since?

"1. In this theory it is assumed, that bodies are formed of matter, consisting of globular atoms of different sizes, having an attraction for each other in the direct proportion of their bulk, or quantity of matter, and inversely as the square of their distance; and of light, consisting of globular atoms, constantly separated by a repulsive force, regulated by the same law of distance, uniform in size, and much smaller than the atoms of matter; for which they have an attraction, also regulated, both with regard to distance and dimensions, by the laws already mentioned.

"2. The light under the influence of these laws must surround the atoms of matter, forming what we have called an atmosphere about each atom, and this atmosphere will be held in its position by various degrees of force, which will draw the atoms of light nearer to each other as they approach the atom of matter, and thus give it greater intensity (density). The atmosphere, it is supposed, will be divided into strata of different intensities, forming concentric spheres; every atom of light at equal distances from the centre being held in its position by equal forces."

"3. The point of saturation, under ordinary circumstances, will be when the repulsive force of the light is sufficient to counteract the attraction of the central atom of matter; when, with the exception of the attraction of the atoms of matter for

* Abstract of a series of papers, entitled "Outlines of a theory intended to connect the operations of nature upon general principles."

each other, forming the force of gravitation, the atoms will be neutral; the opposing forces of attraction and repulsion being equal."

Hence as these forces are obedient to the same law of distance, light, whether disengaged in the form of caloric, or united in a more compressed state to the atmosphere of an atom of matter, must be imponderable; the attraction of the surrounding matter being everywhere counteracted by the repulsive force of the light attached to it.

My theory, it will be observed, differs from M. Mossotti's, among other particulars, in confining the repulsive force to the atmospheres of light which surround the atoms of matter; but as these atmospheres are connected with their respective atoms by a powerful attraction, the repulsive force of contiguous atmospheres will be equivalent in effect to a repulsion between the atoms themselves.

Every known operation of nature may be inductively traced from the simple principles assumed in this theory without the slightest deviation from the laws assigned to matter and light, *its only agents*. But by far the most interesting and important part of these phænomena are produced by the derangement of the atmospheres of light which surround the atoms of matter, arising from their repulsive action upon each other. The quantity of light attached to each atom is not materially altered by this derangement; but, as the neutral state of the atoms is derived from an exact equilibrium between the attractive and repulsive forces, to which an uniform state of their atmospheres is essential, the unequal distribution of the light which forms these atmospheres must give them polarity, by rendering the part of the atmosphere where the light is in excess, positive, in comparison with the part of the atmosphere of the same atom from which this excess is taken, and which must be in an equal degree negative. The polarity thus induced, which requires the contiguity of dissimilar atoms, or atoms the atmospheres of which differ in density, and consequently in electrical force, is the foundation of the attraction by which these atoms are united when in a state of cohesion; and a similar polarity, induced by the action of contiguous masses, or bodies of atoms thus united, which also for this purpose are required to exhibit to each other surfaces of light of different intensities, is the foundation of all the phænomena of electricity. I remain, Gentlemen, yours, &c.

Bawlish, Shepton Mallet, PAUL COOPER.
April 10, 1837.

P.S. I am preparing a paper in which these principles will be applied to the explanation of the peculiar condition of iron discovered by Dr. Schœnbein; and which, in connexion with

this subject, will include an explanation of electro-chemical action generally.

[NOTE.—We may take this opportunity of referring those who are interested in the philosophy of molecular action, to the account of the theories of Father Boscovich and Mr. Michell, in Dr. Priestley's Disquisitions on Matter and Spirit, vol. i. p. 38, &c.; and in his Correspondence with Dr. Price, pp. 47, 243, &c. This account, we think, may be advantageously read in connexion with the memoir of M. Mossotti. In a Memoir on the Absorption of Light, the continuation of which will appear in the next Part of SCIENTIFIC MEMOIRS, the Baron von Wrede says, "We must not be thought too bold when we suggest that by observations on the absorption or light we may find a new way opened to us of viewing the constitution of matter which may perhaps lead to results that could be attained in no other way."—EDIT.]

LXX. *On the Action of Electricity on Albumen. By* GOLDING BIRD, *Esq., F.L.S., F.G.S., Lecturer on Experimental Philosophy at Guy's Hospital.**

To Richard Phillips, Esq., F.R.S., &c.

DEAR SIR,

I FEEL exceedingly indebted to you for pointing out to me the observations of M. Lassaigne on the coagulation of albumen by electric currents, in the *Annales de Chimie.* From this paper it appears that M. Lassaigne applied a similar explanation to the coagulating influence exerted by electric currents on free albumen to that which I ventured to propose in a late number of your Magazine*. The original paper in the *Annales de Chimie* is very brief, and the author's hypothesis is expressed in the following words:

"(L'albumine) la plus pure qu'on puisse se procurer provient de l'œuf: encore celle-ci contient-elle une petite quantité de chlorure de sodium. Il doit nécessairement arriver, lorsqu'on soumet une pareille solution à l'action de la pile, que la petite quantité de sel qu'elle renferme se décompose de manière que l'acide se porte vers le pole positif, tandis que sa base est attirée vers le pole négatif.........: donc l'albumine mise en contact avec le pole positif, où vient se rendre l'acide, doit se combiner avec celui-ci et se précipiter." (*Annales de Chimie et de Physique*, tome xx. p. 98.)

M. Lassaigne mentions the coagulation of albumen at the positive electrode *only*, and makes no reference to those cases in which it takes place at the negative electrode, as in Mr. Brande's experiments referred to in my last communication.

* See vol. ix. p. 109, and p. 84 of the present volume.—EDIT.

The only experiment detailed by Lassaigne in *direct* support of his hypothesis is by no means satisfactory, involving as it does the necessity of the *solubility of albumen after its coagulation by alcohol in distilled water*; still as the experiment is interesting and worthy of repetition, I shall, in justice to the talented chemist who performed it, take the liberty of copying the recital of it.

"....Le moyen que nous avons, employé pour y parvenir a été la coagulation du blanc d'œuf par l'alcool à 28°, et son lavage à plusieurs reprises jusqu'à ce que la dissolution d'argent n'y démontrât plus la présence du chlore.

"L'albumine ainsi traitée a été mise avec de l'eau distillée; une petite quantité s'y est seulement dissoute, car la solution précipitait par l'acide nitrique l'infusion de noix de galle, et était troublée par la chaleur.

"Nous avons placé cette solution dans un tube de verre recourbé en siphon, et nous l'avons soumise à un courant galvanique: elle ne s'est nullement troublée; mais après y avoir ajouté quelques gouttes d'une solution de chlorure de sodium, nous avons observé au pole positif qu'elle est devenue laiteuse, et qu'elle a déposé des flocons blancs." (*Ann. de Chim. et de Phys.* tom. xx. p. 99.)

From these observations it appears that in the *theory* of the action of electric currents on albumen I have been anticipated; which is rather gratifying to me than otherwise, as I trust that the experiments detailed in my last communication are sufficient, if not to prove, at least to render highly probable, the correctness of the hypothesis proposed by M. Lassaigne in the paper above alluded to.

I remain, dear Sir, yours truly,

22, Wilmington Square, April 4, 1837. GOLDING BIRD.

LXXI. *Description and Use of an Electro-magnetic Balance, and of a Battery with invariable Currents. By* M. BECQUEREL.*

HITHERTO we have possessed only two modes of comparing the relative intensities of electric currents. One consisting in making a magnetic needle oscillate for a given time, at the same distance, from a conducting wire traversed by currents possessing different degrees of energy, and then calculating the intensity of each by means of the formula for the pendulum; the other requiring the employment of the galvanometer.

Neither of these methods enables us to refer the intensities of a current to a common measure that may be obtained with

* From the *Comptes Rendus de l'Académie Royale des Sciences* for 1837, No. 2, &c.; being an abstract of a paper read before the Academy, Jan. 9.

facility, although this is an object which we should keep constantly in view while studying the action of the electric forces.

I have endeavoured to compare the electro-magnetic effects of a current by means of weight. The following is a description of the apparatus I employ. I take an assaying balance so delicate that it will be turned by a fraction of a *milligramme**. At each end of the beam there is suspended from a vertical pin a scale pan with a magnet having its north pole hanging downwards. I then fix upon an apparatus properly constructed, two glass tubes of such a bore that the two magnetic bars may be passed into them without touching their concave surfaces. Around each of these tubes there is wound a copper wire covered with silk, and so long as to form ten thousand circumvolutions. After having placed the bars in the direction of the axis of the spirals, I cause an electric current to pass through the wire. Let us, at first, observe a single spiral; it is evident that the magnetized bar, as well as the beam with which it communicates, will rise or fall according to the direction of the current. Let us now, so place the second spiral that the beam will move in the same direction, when the wire is traversed by the current, and bring the two spirals to communicate with each other; the actions of *both* upon the bars will then necessarily be excited. The use of the apparatus will be best illustrated by a few examples. Having taken two plates (one of zinc and the other of copper), each presenting a surface of 4 square centimetres (= 1·6 inch nearly), and being in communication with the two spirals, I immersed them simultaneously into ten grammes of distilled water: one of the scales rose, and it was found necessary to place in it a weight of 2^{m}·5 (= ·0385 grain) in order to restore the equilibrium. The magnetic needle of a short-wired multiplier, which had been placed in the circuit, made a deviation of 60 degrees. A drop of sulphuric acid being added to the liquid, it was found necessary to employ 35^{m}·5 (= ·546 grain) in order to preserve the equilibrium. The two currents were therefore nearly in the ratio of 1 to 14.

I subsequently endeavoured to ascertain in weight the ratio of currents issuing from batteries composed of elements more or less numerous. With a pile of 40 elements charged with water, containing $\frac{1}{60}$ sulphuric acid, $\frac{1}{20}$ sea salt, and some drops of nitric acid, 615 milligrammes (= 9·471 grains) were required to preserve the equilibrium. Hence it follows that this current is to the current obtained by means of a single pair in the ratio of $17\frac{1}{2}$ to 1.

For the purpose of measuring the thermo-electric currents,

* ·0154 grain.

spirals similar to those just mentioned, except that they consisted of two sets of circumvolutions, have been employed. I have employed them for the purpose of determining the temperatures of the several layers of the flame of an alcohol lamp by means of two platina wires of different diameters joined at one end. The temperatures were found to be 1310°·98, 913°·24, 743°·50.

The instances adduced in this memoir are decisive as to the facility with which, by means of weight, we may compare the intensities of currents produced by electricities of different tensions.

If we wish to measure the continuous action of a force, we must first endeavour to give it an unvarying intensity. But the electric current produced by the common piles, and even by a single pair, is liable to continual variations, which render it impossible to make its mode of action the subject of calculation. In order to obviate this inconvenience I have constructed a pile which produces a current whose intensity suffers no sensible variation for twenty-four, and in some instances for forty-eight hours.

Some years since I made public a very simple apparatus producing a current varying but little during a given time. It consists of two small glass phials; one containing concentrated nitric acid, and the other a solution of caustic potash, also concentrated. The two phials communicate with each other by means of a bent glass tube, filled with very fine clay moistened with a solution of sea-salt. Into the phial containing the alkali I plunge a plate of gold, and into the other a plate of platina. When the two plates are brought into communication with a multiplier, a current of considerable energy is found to result from the reaction of the acid on the sea-salt and the potash. The plate of gold takes the negative electricity to the alkali, and the plate of platina carries the positive electricity to the acid.

In order to obtain the maximum effect, due attention must be given, in the construction of this apparatus, to the following considerations. Were it possible to transform into a current all the electricity that is disengaged in the combination of a given quantity of acid with a proportionate quantity of alkali, this current would, in its turn, decompose all the salt that is formed. Accordingly, if in the reaction of an acid on an alkali we can direct a sufficient portion of the electricities disengaged, we shall have a current of sufficient intensity to produce decompositions. For the purpose of partially attaining this condition, we take two tubes of platina, each being bent at one end, in order that it may be inserted into a glass tube. One of the platina tubes is filled with clay

moistened with nitric acid; the other with clay moistened with a solution of potash, and the intermediate glass tube with clay moistened with a solution of sea-salt. The lower ends of the platina tubes are closed with covers of the same metal pierced with a great number of small holes. This end of the tube, which is filled with the clay moistened with acid, is plunged into nitric acid, and the same end of the other into a solution of potash. In order to facilitate the transmission of the electricity from the clay to the sides of the tubes, the clay is mixed with a certain quantity of finely divided platina, for the purpose of increasing its conducting power.

When those arrangements are made, platina wires are attached to the extremities of the bent parts of the tubes for the purpose of transmitting the current through bodies. By combining several apparatus of the same kind we have a pile the effects of which are constant.

One of these pairs required $8^{m}\cdot 5$ (= ·1232 grain) to prevent the scales from turning. A galvanometer with a short wire being placed in the circuit, exhibited during the same time a deviation of 79°. In my memoir I have shown that during a considerable space of time the effects of this pile underwent no sensible variation. It is easy enough to account for this invariability in its effects. We know that the decomposing metallic plates forming a part of a voltaic circuit, and when plunged into a solution, are polarized in such a manner as to produce a current in a direction contrary to that of the pile. The polarization of each of these plates is manifested in the deposition of a substance,which is transferred to its surface by the current, and the nature of which depends on the position of this plate with respect to the extremities of the pile. So long as this substance remains in contact with the plate, there is a current in a direction contrary to that of the original current. But if the substance is surrounded by a body having a close affinity with the substance, it enters into combination with it, and the plate is immediately unpolarized. Such is precisely the case with the different elements of the pile which we describe. The alkali which is transferred to the negative plate combines immediately with the surrounding acid, and the alkali deposited on the positive plate is neutralized by the acid which surrounds it.

I have entered into some detail respecting the electro-chemical effects of the polarization of the decomposing plates, when they transmit constant currents produced by an apparatus consisting of one, two, three, and four pairs. I then set forth the result of the first experiments, which I made with the kinds of apparatus already described, in order to establish

the relations by which affinities are connected with the electric forces. Since the discoveries made by Mr. Faraday on the nature and extent of electro-chemical decomposition, we know that the chemical power of a current is in the direct ratio of the absolute quantity of electricity which is in motion. It is by resting on this principle that he succeeded in his endeavours to determine the equivalents of bodies; but in his researches he disregarded the absolute intensity of the force that is in action at each instant. This is the defect which I have sought to supply by means of my apparatus. It has long been remarked that the elements which are combined with the greatest energy are also decomposed with the greatest readiness by currents, and that the elements which are combined in consequence of feeble affinities are those which offer the most obstinate resistance to the decomposing action of electricity in motion. It seems to follow from this circumstance that all compound bodies are dissolved under the influence of a current proportioned to the force of the affinity by which their elements are united. If then we could establish a relation between the intensity of this current and affinity, we should be enabled to measure the latter. In researches of this kind due attention must be paid to the following observations of Mr. Faraday: 1st, that the electric powers as well as the chemical action of electricity are definite; 2nd, that a considerable quantity of electricity in the form of a current decomposes but a small quantity of elements; 3rd, that the electric agent is employed only for the purpose of overcoming the electro-chemical powers; whence it may be inferred that the quantity which passes is at least equal to the quantity possessed by the separate molecules; 4th, that there is a perfect accordance between the theory of definite proportions and that of electro-chemical affinity, whence it follows that the equivalent parts of bodies may be considered as volumes possessing equal quantities of electricity, or at least equal electric powers. The atoms of bodies which are equivalent to each other in their ordinary chemical action have therefore equal quantities of electricity combined with them. The following are the experiments which I made in order to arrive at the solution of the problem which I proposed to myself.

When a constant current is made to pass into two differently saturated solutions of a salt with a reducible base, the quantity of salt decomposed is exactly the same in both. I took $2^{grm}\cdot8$ (= 43·25 grs. nearly) of dry nitrate of copper and dissolved it in $10^{grm}\cdot3$ (= about 6 fluidrachms) of water: half the solution was diluted by a quantity of water equal to it in volume; the two copper wires plunged into the two negative

branches weighed each $0^{grm}\cdot3385$ (= 5·0775 grs.). After forty-eight hours these wires weighed $0^{grm}\cdot36$ (= 5·4 grs.): they had therefore gained in weight $0^{gr}\cdot0215$ (= ·3225 gr.). The intensity of the current which produced this effect was represented by 5 milligrs. (= ·077 gr.). The intensity of the current being diminished by one half, the quantity of copper reduced in forty-eight hours was found equal to 0·01, that is, to half the quantity reduced in the preceding experiment.

The same wire and the same solutions were submitted during forty-eight hours to the action of a current which counterbalanced 3 milligrs. (= ·0462 gr.): the quantity of copper obtained was $0^{m}\cdot012$. Now, if the quantities of copper reduced in the two experiments be compared, they are found to be exactly proportional to the intensities of the current. Several experiments of the same kind were made with solutions of nitrate of silver by varying the density of these solutions and the intensity of the current. The quantities of metal reduced were exactly proportional to the variations of the current, the source being constant; the constancy of the source being an indispensable condition.

These results follow from the observations of Mr. Faraday. But between his results and those which I have just stated there is this difference, that he has disregarded the absolute intensity of the current, whilst I take it into account. We shall show in another memoir the advantages derived from this new element introduced into experiments connected with electro-chemical researches.

We have endeavoured to ascertain by means of the electro-magnetic balance, the proportions in which the quantities of reduced metal are found, when solutions of several metals are subjected to the action of the same current, of a known intensity. Three solutions, one of copper, another of silver, and a third of zinc, were introduced into the circuit. These solutions were placed in tubes of the form of a U, and each of them was in contact on the negative side with a plate of platina, and on the positive side with a plate of the same metal as that in solution. They were subjected to the action of an apparatus consisting of two pairs prepared with the cylinders of platina: the following are the results obtained.

The intensity of the currents balanced a weight of $5^{m}\cdot5$ (= ·0847 gr.). After twenty-four hours the silver precipitated weighed $0^{m}\cdot305$; the weight of the copper precipitated was $0^{m}\cdot090$; that of the zinc precipitated was $0^{m}\cdot0925$. Now if we consider the proportion of the three quantities of metal precipitated, we shall find that they are proportional to the atomic weights of the silver, the copper, and the zinc, inasmuch

as 305 (the weight of the first) is to 90 (the weight of the second) as 108 (the atomic weight of the silver) is to 31·8, instead of 31·6 (the atomic weight of the copper). In like manner, 305: 92·5::108 (the atomic weight of the silver) is to 32·8 (the atomic weight of the zinc,) instead of 32·5, found by Mr. Faraday. It is evident, then, that the apparatus with a constant current, and consisting only of two pairs, together with the electro-magnetic balance, enables us to find the atomic weights of metals, and to determine the quantities of reduced metal that correspond to a given intensity of current.

LXXII. *An Experiment on the Interference of Light.* *By* H. F. Talbot, *Esq.*, F.R.S.*

I BELIEVE the following experiment to be a new one, and it seems to afford a satisfactory illustration of the theory.

Make a circular hole in a piece of card of the size of the pupil of the eye. Cover one half of this opening with an extremely thin film of glass (probably mica would answer the purpose as well, or better). Then view through this aperture a perfect spectrum formed by a prism of moderate dispersive power, and the spectrum will appear covered throughout its length with parallel obscure bands, resembling the absorptions produced by iodine vapour.

The cause of this phænomenon probably is, that one half of the light which passes through the glass film his its undulations thereby retarded by a certain quantity, which may be called A.

Let L be the length of the undulation of any coloured ray, which I suppose to be a much smaller quantity than A.

Then if we consider the colours in succession, L increases progressively from the violet to the red. Consequently the quotient $\frac{A}{L}$ becomes by turns a whole number and a fraction, and then again a whole number, and so on alternately a great number of times. Whenever $\frac{A}{L}$ is a whole number, the two halves of the light agree in the phase of their undulation. But when $\frac{A}{L}$ is midway between two whole numbers, the two portions of light are opposed in phase, and therefore the corresponding colour cannot make its appearance in the spectrum at all; and therefore also a dark band appears in the place it would have occupied.

* Communicated by the Author.

LXXIII. *On the Carboniferous Series of the States of New York and Pennsylvania.* By Thomas Weaver, *Esq., F.R.S., F.G.S., M.R.I.A., &c. &c.**

IT is only very lately that my attention has been drawn to the observations of Mr. R. C. Taylor on the Carboniferous Series of the United States of North America, published in the Lond. and Edin. Phil. Mag., for December 1836, and which have reference to my notice on the same subject inserted in the same Journal for August 1836. As the author does not in all cases appear to have entered into the full scope of my argument, or to have wholly considered the reasons of my persuasion that Professor Eaton had established the true position of the anthracitous coal formation in the north-eastern part of Pennsylvania; observing also (p. 410) that "he does not know which of the calcareous rocks is meant by Prof. Eaton as the limestone which supports the strata containing the Pennsylvanian coal," perhaps a few additional remarks may serve to throw some further light upon the subject.

In the notice referred to, I have stated that in the northern part of the State of New York, the progression from north to south, in the ascending order, and extending into Pennsylvania, is as follows,—all the beds being in conformable position with a general dip to the southward: 1. old red sandstone; 2. carboniferous limestone; 3. coal measures, distinguished by the prevalence of red shales and red sandstones, with beds of sandstone conglomerate and of limestone, the whole forming an alternating series, in which coal appears hitherto to have been rarely met with; 4. productive coal measures, containing abundant deposits of anthracitouscoal in the coal-fields of Carbondale and Lackawanna and Wyoming on the Susquehanna, and of bituminous coal in the coal-fields of Bradford, Tioga, Lycoming, and Clearfield.

The main body of the carboniferous limestone (No. 2.), resting on the old red sandstone (No. 1.), extends from the northern extremity of Lake Erie, in an easterly direction, through the State of New York, to the Helderberg mountain, situated about 20 miles to the S.W. of Albany. Here its course is inflected to the S. and S.W., and Prof. Eaton states that he has followed this limestone upon that range 120 miles from the Helderberg mountain, extending into Pennsylvania on the right bank of the Delaware river, and being flanked throughout in this direction by transition rocks on the east†. The

* Communicated by the Author.

† Geological Text-Book, 2nd edit., pp. 66, 67.

carboniferous limestone thus appears to support not only the bituminous coal measure series of the Alleghany and Catskill mountains on the N. and the E., but also to inclose and support on the E. the anthracitous deposits of Carbondale, Lackawanna, and Wyoming; in fact, serving as a base to the alternating series No. 3, which supports the more productive coal-bearing measures No. 4*. The further continuity of the anthracitous range from Wyoming to the southward, through the regions bordering on the Lehigh and Schuylkill rivers, and their probable juxtaposition in that direction with transition rocks on the east, I have already adverted to in my notice of August, 1836.

In the State of New York, the southern border of the main body of the carboniferous limestone intersects upon its range the Lakes Seneca and Cayuga, and near the heads of those lakes, on the south, the coal measures No. 3 contain thin layers of bituminous coal †. The same occurrence has been remarked in Otsego county in the shale lying above the limestone ‡. In the same manner narrow seams of bituminous coal have been found in the Catskill range bordering on the river Schoharrie, and again in the southern part of the same range in Ulster county, varying from 8 to 22 inches thick, the beds being in some places horizontal, but in general slightly inclined to the west§. To what extent other seams of coal may occur in the higher accumulation of these coal measures No. 3, subjacent to the great anthracitous and bituminous deposits of Pennsylvania in series No. 4, remains yet to be proved.

If to these circumstances of relative position we add the further consideration that fossil plants are found both in the anthracitous and bituminous coal-fields of Pennsylvania, which are identical with those occurring in the bituminous coal-fields of Ohio and in the great coal-fields of Europe||; that both the anthracitous and bituminous coal regions of Pennsylvania are alike productive of large quantities of clay ironstone; and moreover that, so far as it has been shown, the alternating series No. 3, and the subjacent carboniferous limestone No. 2, exhibit such other organic exuviæ as are common in the carboniferous epoch; combining these several data, I do not see how we can avoid coming to the conclusion that the whole series, from No. 1 to No. 4 inclusive, belong to the great carboniferous order, and that no part of that series belongs to the transition system.

* Geological Text-Book, pp. 90, 121, 124.

† Ib., pp. 79, 110.

‡ Ib., p. 121.

§ J. Pierce in American Journal of Science, vol. vi. pp. 94 to 96.

|| Geological Text-book, pp. 91, 125; and American Journal of Science, vol. xxiii. pp. 399, 400.

This leads me again to the consideration of the alternating series in Pennsylvania, composed largely of red sandstones and red shales, with beds of sandstone conglomerate and of limestone, and some bituminous coal, mostly arranged in double anticlinal and synclinal order, as exposed to observation between the bituminous coal ranges of the Alleghany mountains on the west, and the anthracitous coal region of Schuylkill, &c. on the east. Had not Mr. R. C. Taylor denominated this alternating series *transition*, and the red sandstone immediately underlying the Alleghany bituminous range *old red sandstone*, judging merely by the evidence produced, and reasoning from analogy, I should have been induced to consider them as the prototypes or representatives of the class of beds which appear in the northern face of the Alleghany mountains, and which I have designated as No. 3; both in fact appearing to form the immediate support of the more productive coal-bearing measures No. 4. No evidence is given by the accompanying fossils to prove that this alternating series belongs to the transition period. On the other hand, it is admitted by the author (p. 409.) that this alternating series comprises four or five troughs or basins containing coal, which on the eastern side of the State of Pennsylvania is anthracitous, and on approaching the S.W. contains upwards of 16 per cent. of bitumen and volatile matter; while some of the anthracitous beds pass into bituminous coals in certain places (p. 408). This fact affords an argument on the other side, as no well authenticated instance exists, so far as my knowledge extends, of a bed of bituminous coal having ever yet been found within the limits of the transition system.

This is not a question of mere theoretical speculation, but one of high practical importance, as connected with geological investigation and the œconomical purposes of life; and from the zealous researches now understood to be in progress, both in the State of New York and in Pennsylvania, we may reasonably expect that a fuller light will soon be shed upon the subject; in effecting which the successful labours of Mr. R. C. Taylor will no doubt appear conspicuous.

To what extent transition rocks may occur in Pennsylvania within the area circumscribed by the anthracitous range on the east, the bituminous range on the west, and by both conjointly on the north, remains yet to be proved. Should such appear within that space, it may require some caution not to confound simple contact with a portion of the carboniferous series, with an arrangement coordinate with the latter.

A clear exposition of all the relations of the transition system

within the United States is a great desideratum, toward the accomplishment of which the attention of American geologists cannot be too sedulously directed.

April, 1837.

LXXIV. *On the Identity of two Minerals from Vesuvius named Biotine and Anorthite, and on a new Variety of Hemitrope Crystal of Quartz.* *By* H. J. BROOKE, *Esq., F.R.S., &c.**

[With Figures: Plate III.]

IN a paper in the last Number of this Journal, p. 278, on the regular crystalline composition of two different minerals, allusion is made to the combination of Felspar and Cleavelandite in crystals, which, however, by an error of the press, are said to be from *Bavaria* instead of *Baveno.*

The exact relations of the crystals of these two minerals and of Anorthite have not, that I am aware of, been exactly pointed out so as to explain the combination referred to, and I am therefore induced to request the editors to allow a space for the figures alluded to below.

Fig. 1. (Plate III.) is an oblique rhombic prism, now adopted as the primary form of Felspar. The small figure marked P M T, the faces of which are parallel to the cleavage planes, was regarded by Haüy as the primary, and from the relation which subsists between these two figures, the secondary forms of felspar might be derived from either.

Assuming for the present purpose the small figure, enlarged and represented by fig. 2, as the primary, the following analogy and differences of angles will be found to subsist between Felspar, Anorthite and Cleavelandite.

	P on T.	P on M.	T on M.
Anorthite	86°	110° 40′	117° 30′
Felspar.............	90	112	120 35
Cleavelandite ...	93 30′	115	119 30

The crystals of Cleavelandite are formed over the planes T and M of Felspar, with their axes parallel to the intervening edge.

Fig. 3 represents a regular crystal of Anorthite, and figg. 4 and 5 two crystals of the variety named Biotine. The crystal of Anorthite is lengthened proportionally in the direction of its axis, and fig. 4 of Biotine very disproportionally in the direction of the oblique diagonal. It appears from M. Monticelli's figures that he has considered the planes T, *n*, P, *e*, as

* Communicated by the Author.

those of a prism distinct from Anorthite, and has assumed the others to be terminal planes. But the correspondence of the planes marked by similar letters in the figures may be ascertained by means of the reflective goniometer, so as to remove all doubt of the identity of the two minerals. Many of the crystals named Biotine are flat, and have apparently square terminal planes as shown in fig. 4, which is a remarkably deceptive crystal, and without reference to the goniometer might easily be supposed not to belong to Anorthite.

A crystal of quartz in the writer's cabinet, resembling fig. 8, and said to have come from Dauphiné, presents a hemitrope form differing from any hitherto described. In carbonate of lime the axis of revolution of twin crystals is parallel or perpendicular to the crystallographic axis, or perpendicular to a primary plane, or to a tangent plane on a primary edge; and in all other cases it has been found parallel or perpendicular to an axis, or perpendicular to a primary edge or plane, or to a *single* plane produced by some *simple* law of decrement. Indeed so universal have these relative positions of the axes of revolution been in all the instances hitherto described, that they have been regarded by Haüy and others as fundamental laws of this kind of structure. Haüy's expression (Cryst., vol. ii. p. 273) is, "Le plan qui est censé avoir partagé le crystal original en deux moitiés est *toujours* parallèle, soit à *une des faces du noyau*, soit à une face produite en virtue d'une *loi simple* de décroissement sur les *bords* ou sur *les angles* du même noyau."

The axis of revolution of the crystal fig. 8 is perpendicular to *one* of *a pair of planes* replacing an edge of the primary rhomboid, resulting from a *complicated intermediary* law, expressed, according to the notation of Haüy, by ($B 1\ D\frac{1}{2}\ D\frac{1}{3}$), and which would produce tangent planes on the edges of the pyramid of the common crystals of quartz. Fig. 6. shows the position of these planes a, a', a'', on the primary rhomboid; and fig. 7. shows their relation to the hexagonal pyramid. These planes a do not occur on the hemitrope crystal, fig. 8, but as the axis of revolution is perpendicular to the edge $d\,c$, it must consequently be perpendicular to a tangent plane replacing that edge.

Assuming 94° 15′ as the angle of the rhomboid of quartz, the inclination of the edge $d\,c$ of the pyramid on the axis will be 42° 16′; and consequently the angle $d\,e\,d'$ will be 84° 32′.

H. J. B.

LXXV. *On the* Rev. J. G. MacVicar's *Experiment on Vision.* *By* J. T. Graves, *Esq., A.M.**

THE ordinary laws of reflection and perspective are sufficient to account for the radiated appearance described by the Rev. Mr. MacVicar (*antè*, p. 234,) without the hypothesis of any hitherto unknown symmetrizing power in the eye.

No radiation is observed when the particles are spread at whatever distance over a non-reflecting surface, or are placed in *immediate contact* with a reflecting one. Now if a reflecting surface in the required position of contiguous parallelism to the plane of the dust contributes to the success of the experiment, it is surely *à priori* likely that it does so rather by the addition it makes to the objects of vision than by any induced alteration in the mechanism of the eye, affecting the mode in which the former objects, if seen alone, would be viewed. Accordingly we are led to consider the actual nature of the picture before us when the phænomenon is observed.

When we survey with one eye dust spread over a common looking-glass, we see not only the particles themselves but their *images* reflected by the mercury (wherever those images are not intercepted by the position of other particles on the glass), and both classes of objects, from their general similarity of appearance, are referred to the same plane. Upon further examination we shall perceive that every reflected image when referred to the external surface of the looking-glass, appears in a direct line between its prototype and the point of that external surface nearest the centre of the eye. In making this observation, we may use with advantage small shots or seeds instead of flour or other finely-powdered substances, and we may put out of consideration the fainter and scarcely perceptible images formed by reflection at the external surface, as they do not materially alter the general picture. Now the appearances above described might have been predicted as the necessary results of known principles.

Within the ordinary limits of the sphere of sight, it is an observed approximate law of vision, whether direct or indirect, that when a pencil of rays diverging from any point enters the pupil, an image is seen in the direction of the line drawn from that focal point through the centre of the eye. Again, it is an observed law of light that the angles of incidence and reflection are equal and in the same plane. From the latter law it follows by simple geometrical reasoning, that all the rays proceeding from any point and reflected by a

* Communicated by the Author.

plane mirror diverge after reflection from an opposite corresponding point equidistant from the mirror; and it also follows that the line joining the conjugate points from which direct and reflected pencils diverge is perpendicular to the plane of the mirror. Hence if we have a perspective plane parallel to the plane of the mirror, and carrying all the objects to be reflected, the line of direction in which, by the preceding law of vision, any reflected image is seen will cut the line which joins the object and the point of the perspective plane nearest the centre of the eye, in the ratio which twice the distance of the perspective plane from the mirror bears to the distance of the same plane from the centre of the eye. While the head remains unmoved, the rolling motion of the eye does not affect the position of its centre, and consequently the apparent positions of the reflected images as well as those of their direct objects remain unaltered.

The relative position of the particles and their images to the point of the perspective plane nearest the centre of the eye will account for the radiated appearance. The interval between each particle and each image produces a short line pointing in a given direction. The multitude of such *pointers* and the multitude of instances in which separate pointers combine to form a prolonged continuous line radiating to a given centre, actually produce in the spectrum a predominant symmetry, which the mind, without any previous peculiar adjustment of the eye, can scarcely fail to notice.

We may form a similar radiating group of objects of vision, which shall not have the disadvantage of being altered as to half its components by every motion of the centre of the eye, if we first dot a sheet of paper indiscriminately, and then mark a second system of dots so related to the former that the interval between each pair of conjugate dots may tend in direction to an assumed centre and be proportional in length to its mean distance from that centre. Here it is evident on inspection that no extraordinary symmetrizing power is required to perceive the radiation. We may observe, however, that if we mark the second system only to a certain distance, around the assumed centre, a principle of continuity will incline us to trace the resulting radiation beyond that distance, by drawing our chief attention to the exterior points in the original system in so far as they continue the discovered symmetry. Again, if we mark the first system of dots in blue, and the second in red, the radiation resulting from the combined position of the systems will fix our attention on the parts of each system in so far as they contribute to the joint effect, so that when the two systems are simultaneously viewed as separate

wholes, they will each appear to radiate. If this were repeated often enough, we might perhaps afterwards be able to trace a radiation in either system unbacked by the guidance of the other, for the eye is predisposed to recur to that *attitude* in which while previously looking at an object symmetry has been perceived.

The eye has parts severally capable within certain limits of distinct adjustments voluntary or instinctive. From natural conformation, acquired habit, or the present immediate operation of metaphysical causes, the eye seeing simultaneously the whole of a given field, may probably possess a superior facility of severally adjusting its mechanism by parts corresponding to parts of the field in some manners rather than in others, and may also be predominantly inclined to alter its sphere of vision in particular directions. Hence in a group of objects in which different laws of arrangement are discoverable, one attitude of the eye may be the best adapted for the discovery of one law and another for that of a different one. For example, let a sheet of paper be dotted in such a manner that each surrounded dot may be the centre of a regular hexagon, of which the surrounding dots mark out the apices; then the rows, in which the mind will *without effort* arrange the dots, will vary with the direction of the eye, and, *with effort*, the grouping may be altered without altering the field of vision. A similar observation may be made in looking at large piles of cannon balls. Not the imagination merely but the plastic eye of Phidias is at work while he is delineating the Venus in the yet uncarved marble before him. That visual *prejudice* which consists in a tendency to adjust the respective parts of the eye in particular modes, not naturally suggested by the general impression resulting from the objects presented to those parts, is a defect exemplified in persons who see ghosts in the window-curtains and faces in the burning embers.

Though the interesting experiment which Mr. MacVicar has introduced to the public in the pages of this Magazine, does not seem adapted to illustrate the changes that take place in the eye, corresponding to the changes that take place in the grouping and symmetry seen in the subjective spectrum of a given object, it may be of use for a purpose of scarcely inferior curiousness and importance; for if we compare what we see in surveying the powdered looking-glass with *both* eyes, with the phænomena which we should expect to result from the separate and *different* spectra due to each eye singly, we may perhaps be enabled to throw light on some obscure processes in the physiological mechanism of binocular vision. This, however, is an inquiry deserving consideration by itself.

For the present let it suffice to have indicated the general causes of the radiated appearance seen in Mr. MacVicar's experiment. J. T. GRAVES.

[Communications on the same subject have been received from Mr. H. S. Peacock of London, Mr. William George Horner of Bath, Mr. Robert Wilson of Glasgow, Mr. J. De C. Sowerby of Camden Town, Mr. G. Dodd of London, and Mr. G. H. Hoffman, Surgeon, of Margate. They all agree in referring the phænomenon in question to the principles of reflection and perspective explained in the preceding paper, and the three latter gentlemen respectively mention that they had observed the radiated appearance for considerable periods before the publication of Mr. MacVicar's letter.—EDIT.]

LXXVI. *On the Composition of the Right Rhombic Baryto-Calcite, the Bicalcareo-Carbonate of Baryta of* Dr. Thomson. *By* JAMES F. W. JOHNSTON, *A.M., F.R.S.E., Professor of Chemistry and Mineralogy in the University of Durham.**

IN the sixth volume of this Journal, p. 1, I described a mineral occurring in the lead-mine of Fallowfield near Hexham, Northumberland, which I had found on analysis to be a true baryto-calcite, though having a form wholly irreconcileable with the doubly oblique prism of the original baryto-calcite of Brooke.

Since the publication of this description, Dr. Thomson of Glasgow inserted in the Records of Science, vol. i. p. 369, and more recently in his System of Mineralogy, vol. i. p. 141, an account of a mineral from Bromley Hill mine near Alston, having the same specific gravity and the same crystalline form as my new baryto-calcite, but having a different composition.

My attention was first drawn to this circumstance by my friend Mr. Brooke, who at once recognised my second form of baryto-calcite in the mineral of Dr. Thomson, and aware of the importance of a knowledge of the true composition of this mineral to the doctrine of isomorphism, requested to know whether I had actually analysed my specimens from Fallowfield.

My analysis gave the formula $\dot{C}a\,\ddot{C}+\dot{B}a\,\ddot{C}$, that of Dr. Thomson $2\,\dot{C}a\,\ddot{C}+\dot{B}a\,\ddot{C}$, and hence the name *Bicalcareo-carbonate of Baryta* which he has assigned to the mineral.

Unwilling to leave the matter in doubt I repeated my analysis of the Fallowfield mineral.

* Communicated by the Author.

1st. 72·13 grs. dissolved in dilute muriatic acid and the gas made to pass over chloride of calcium, lost 21·18 grs. = 29·363 per cent. of carbonic acid.

2ndly. 50·57 grs. dissolved in muriatic acid, precipitated by fluo-silicic acid and heated to redness, gave of fluoride of barium 29·18 grs. = 32·996 carbonate of baryta, or 65·248 per cent.

The filtered solution was poured into a large quantity of water saturated with sulphate of strontia, and precipitated by sulphuric acid. The precipitate weighed 1·81 gr., and besides strontia contained also the small quantity of baryta held in solution by the fluo-silicic acid. If we neglect this small quantity of baryta, we have 1·456 or 2·87 — per cent. of carbonate of strontia.

A specimen from Bromley Hill, Dr. Thomson's locality, when dissolved in muriatic acid, left 0·16 per cent. of insoluble matter, and gave 29·71 per cent. of carbonic acid.

36·58 grs. precipitated by fluo-silicic acid gave 20·14 grs. of fluoride = 22·774 or 62·156 per cent. of carbonate of baryta.

The sulphate of strontia obtained as before, and containing a little baryta, weighed 3·02 = 8·256 per cent. = 6·641 — carbonate of strontia.

33·05 grs. dissolved in muriatic acid largely diluted and precipitated by sulphuric acid, gave of sulphates* 26·84 grs.

The supernatant solution precipitated by oxalate of ammonia gave 10·01 of carbonate of lime or 30·29 per cent.

The doubly oblique baryto-calcite of Mr. Brooke treated in the same manner gave me of carbonic acid 30·05, of carbonate of baryta 65·97, and of carbonate of strontia 2·317 — per cent.

The following table exhibits these results in connection with

* I have said sulphates, as it is hardly possible to free the precipitate thus obtained from a trace of lime. It also contained sulphate of strontia. 41·27 grs. of the Fallowfield mineral gave me a precipitate weighing 58·88 grs. (it should be in pure $\dot{Ba}\,\ddot{C}+\dot{Ca}\,\ddot{C}$, 56·36). Boiled in nitric acid it lost 3·31 grs.; again boiled 45·94 grs. lost 1·82, and a third time 40·065 grs. lost 0·51. Evaporated to dryness and the dry mass converted into chlorides, I obtained a deliquescent chloride of calcium, tabular crystals of chloride of barium, and prismatic crystals giving the characteristic purple flame of chloride of strontium. Though the last portions of lime and strontia therefore may be dissolved out by repeated boiling in dilute nitric acid, it would appear that the sulphate of baryta itself is not wholly insoluble in this menstruum.

the analysis of Dr. Thomson and the specific gravities of the several specimens analysed.

	Theory.	Fallow-field.	Bromley Hill. Johnston.	Bromley Hill. Thomson.	Doubly oblique from Alston Moor.
Carbonic Acid........	29·625	29·363	29·71	....	30·05
Carbonate of Baryta ..	66·102	65·248+	62·156+	49·31−	65·97+
Carbonate of Lime....	33·898	....	30·29	50·69	
Carbonate of Strontia	....	2·87−	6·641−	....	2·317−
Specific gravity at 60° Fahrenheit		3·694 *3·706	3·70	3·718	3·646

Dr. Thomson informs me that his mode of analysis was to convert the mixed earths into nitrates and digest in absolute alcohol, a method sufficiently simple yet requiring considerable precautions to secure accuracy. It is barely possible that a small portion of the matrix of carbonate of lime on which the mineral rests may have escaped his notice and been mixed with the quantity analysed.

In my former account of this mineral I did not advert to the presence of carbonate of strontia, as I had not attempted to separate it quantitively. It is interesting, however, to find as the above results show, that this substance is capable of replacing, indifferently, either one or both of the isomorphous carbonates of lime and baryta, and in variable quantity. In the oblique rhombic (Brooke's), column 6th, it replaces 2 per cent. of the carbonate of lime, while in the right rhombic it replaces a portion of both, and in the specimen analysed from Bromley Hill about 3 per cent. of each.

Sulphato-carbonate of Baryta.—Among the interesting new barytic minerals described by Dr. Thomson (Mineralogy, vol. i. p.106,) from the same locality,—Bromley Hill mine near Alston,—is one affecting the form of large six-sided prisms terminated by hexagonal pyramids, consisting of 34·3 or one atom of sulphate, and 64·82 or 2·2 atoms of carbonate of baryta. This mineral is said by the dealers to be found abundantly at the above mine; I have not however been able to obtain a specimen of the *true* sulphato-carbonate. In the cabinet of the Natural History Society of Newcastle, are two specimens ticketed sulphato-carbonate, both from Bromley Hill. One of these is in large obscure prisms an inch in diameter composed of a congeries of small ones resting *on a matrix of sulphate of baryta.* This specimen agrees with the description

* After heating to redness, during which it decrepitates slightly and becomes opake, it had a specific gravity = 3·639.

of the mineral by Dr. Thomson, and is said to have been presented by him. I find these crystals to be carbonate of baryta nearly pure, dissolving readily in muriatic acid and leaving an insoluble residue of 0·3 per cent.

The other specimen is in broad flat pyramids, composed also of a congeries of minute crystals; is grayish, has a pearly lustre, and rests on right rhombic baryto-calcite. These crystals were obtained from a mineral-dealer, and are also carbonate of baryta with a little lime. Lime indeed in this district seems to be very generally associated with the barytic minerals. Carbonate of baryta is exceedingly abundant at Fallowfield lead-mine, is generally of a pure white, and was, till lately, collected and exported in considerable quantity to the potteries in Staffordshire. The trade, however, has declined from its being found to contain too much lime.

It would appear therefore that the name bicalcareo-carbonate of baryta must at present be laid aside, being imposed by Dr. Thomson under a misapprehension in regard to the true constitution of the mineral; and that the *sulphato-carbonate, if it exist*, is a much rarer mineral than is generally supposed, and so much resembles the large* crystals of common carbonate of baryta as to deceive the discoverer himself.

Durham, April 14, 1837.

LXXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 222.]

February 2, 1837.—"OBSERVATIONS on the Electro-chemical Influence of long-continued Electric Currents of Low Tension." By Golding Bird, Esq., F.L.S., F.G.S., Lecturer on Experimental Philosophy at Guy's Hospital. Communicated by Thomas Bell, Esq., F.R.S.

The author, after observing that the brilliant discoveries in electro-chemistry obtained by Sir Humphry Davy were effected by the employment of voltaic currents of high intensity, elicited by means of large batteries, adverts to the labours of M. Becquerel, to whom we are indebted for the knowledge of the chemical agency of feeble currents in reducing several refractory oxides to the metallic state†: and also to those of Dr. E. Davy, Bucholz, and Professor Faraday in effecting decompositions of other substances by similar means. In prosecuting this branch of inquiry, the author employed an apparatus analogous to that of Professor Daniell, for obtaining an equal and continuous

* Carbonate of baryta is sometimes met with at Fallowfield in large translucent crystals three or four inches long and two or three in diameter.

† Details of M. Becquerel's researches will be found in Scientific Memoirs, No. III.—Edit.

current of low intensity from a single pair of plates: the metallic solution, in which a copper-plate was immersed, being contained in a glass tube, closed at the bottom by a diaphragm of plaster of Paris, and itself plunged in a weak solution of brine contained in a larger vessel, in which a plate of zinc was immersed; and a communication being established between the two metallic plates by connecting wires. By the feeble, but continuous current thus elicited, sulphate of copper is found to be slowly decomposed, affording beautiful crystals of metallic copper. Iron, tin, zinc, bismuth, antimony, lead, and silver may, in like manner, be reduced, by a similar and slightly modified process; in general appearing with metallic lustre, and in a crystalline form, and presenting a remarkable contrast in their appearance to the irregular, soft, and spongy masses obtained from the same solutions by means of large batteries. The crystals of copper rival in hardness and malleability the finest specimens of native copper, which they much resemble in appearance. The crystallization of bismuth, lead, and silver, by this process, is very beautiful; that of bismuth being lamellar, of a lustre approaching to that of iron, but with the reddish tint peculiar to the former metal. Silver may thus be procured of the whiteness of snow, and usually in the form of needles. Some metals, such as nickel, which, when acted on by currents from large batteries, are deposited from their solutions as oxides only, are obtained, by means of the apparatus used by the author, in a brilliant metallic form. He further found that he could in this way reduce even the more refractory metallic oxides, such as silica, which resist the action of powerful batteries, and which M. Becquerel could only obtain in alloy with iron. By a slight modification of the apparatus he was enabled to form amalgams both of potassium and of sodium with mercury, by the decomposition of solutions of chlorides of those bases; and in like manner ammonium was easily reduced, when in contact with mercury, by the influence of a feeble voltaic current. In this last experiment it was found that an interruption to the continuance of the current, even for a few seconds, is sufficient to destroy the whole of the product which had been the result of the previous long-continued action; the spongy ammoniacal amalgam being instantly decomposed, and the ammonia formed being dissolved in the surrounding fluid.

February 9.—A paper was read, in part, entitled, "On the Elementary Structure of Muscular Fibre of Animal and Organic Life." By Frederick Skey, Esq., Assistant Surgeon to St. Bartholomew's Hospital. Communicated by John Bostock, M.D., F.R.S.

February 16.—The reading of Mr. Skey's paper was resumed and concluded.

The author concludes, from his microscopic examinations of the structure of muscular fibres, that those subservient to the functions of animal life have, in man, an average diameter of one 400dth of an inch, and are surrounded by transverse circular striæ varying in thickness, and in the number contained in a given space. He describes these striæ as constituted by actual elevations on the surface of the fibre, with intermediate depressions, considerably narrower than the dia-

meter of a globule of the blood. Each of these muscular fibres, of which the diameter is one 400dth of an inch, is divisible into bands or fibrillæ, each of which is again subdivisible into about one hundred tubular filaments, arranged parallel to one another, in a longitudinal direction, around the axis of the tubular fibre which they compose, and which contains in its centre a soluble gluten. The partial separation of the fibrillæ gives rise to the appearance of broken or interrupted circular striæ, which are occasionally seen. The diameter of each filament is one 16,000dth of an inch, or about a third part of that of a globule of the blood. On the other hand, the muscles of organic life are composed, not of fibres similar to those above described, but of filaments only; these filaments being interwoven with each other in irregularly disposed lines of various thickness; having for the most part a longitudinal direction, but forming a kind of untraceable network. They are readily distinguishable from tendinous fibres, by the filaments of the latter being uniform in their size, and pursuing individually one unvarying course, in lines parallel to each other. The fibres of the heart appear to possess a somewhat compound character of texture. The muscles of the pharynx exhibit the character of animal life; while those of the œsophagus, the stomach, the intestines, and the arterial system, possess that of inorganic life. The determination of the exact nature of the muscular fibres of the iris presented considerable difficulties, which the author has not yet been able satisfactorily to overcome.

A paper was also in part read, entitled, "On the Function of the Medulla Oblongata and Medulla Spinalis, and on the Excito-motory System of Nerves." By Marshall Hall, M.D., F.R.S. L. and E., &c.

February 23.—The reading of Dr. Marshall Hall's paper was resumed, but not concluded.

March 2.—The reading of Dr. Marshall Hall's paper was resumed and concluded.

The author begins by observing that a former memoir of his, entitled, "On the Reflex Function of the Medulla Oblongata and Medulla Spinalis," published in the Philosophical Transactions for 1833, has been translated into German, and favourably spoken of by Professor Müller, of Berlin*. He states that his object in the present paper is to unfold what he calls a great principle in physiology; namely, that of the special function, and the physiological and pathological action and reactions of the true spinal marrow, and of the excito-motory nerves. The two experiments which he regards as affording the type of those physiological phænomena and pathological conditions, which are the direct effects of causes acting in the spinal marrow, or in the course of the motor nerves, are the following:—1. If a muscular nerve be stimulated, either mechanically by the forceps, or by means of galvanism passed transversely across its fibres, the muscle or muscles to which it is distributed are excited to contract.—2. The same result is obtained when the spinal marrow itself is subjected to the agency

* See our present volume, p. 51 *et seq.*—Edit.

of a mechanical or galvanic stimulus. The following experiment, on the other hand, presents the type of all the actions of the reflex function of the spinal marrow, and of the excito-motory system of nerves, and of an exclusive series of physiological and pathological phænomena :—If in a turtle, from which the head and sternum have been removed, we lay bare the sixth or seventh intercostal nerve, and stimulate it either by means of the forceps or galvanism, both the anterior and posterior fins, with the tail, are immediately moved with energy. Hence the author infers the existence: 1st, of a true spinal marrow, physiologically distinct from the chord of intra-spinal nerves; 2ndly, of a system of excito-motory nerves, physiologically distinct from the sentient and voluntary nerves; and, 3rdly, of currents of nervous influence, incident, upwards, downwards, and reflex with regard to the spinal marrow.

A review is then taken of the labours of preceding physiologists relative to the functions of the nervous system: in which the author criticises the reasonings of Whytt, Legallois, Mr. Mayo, Dr. Alison, and Professor Müller; and illustrates his own peculiar views by several experiments and pathological observations, which appear to him to show that muscular movements may occur, under circumstances implying the cessation of sensation, volition, and every other function of the brain; and that these phænomena are explicable only on the hypothesis that impressions made on a certain set of nerves, which he terms *excito-motory*, are conveyed to a particular portion of the spinal marrow belonging to that system, and are thence reflected, by means of certain motor nerves, upon certain sets of muscles, inducing certain actions. The same actions may also be the result of impressions made directly either on the spinal marrow or on the motor nerves. He accordingly considers that the whole nervous system may be divided into,—1st, the *cerebral*, or the sentient and voluntary; 2ndly, the *true spinal*, or the excitor and motor; and, 3rdly, the *ganglionic*, or the nutrient, the secretory, &c. The excito-motory system presides over ingestion and exclusion, retention and egestion, and over the orifices and sphincters of the animal frame: it is therefore the nervous system of respiration, deglutition, &c., and the source of tone in the whole muscular system. The true spinal system is the seat or nervous agent of the appetites and passions, but is also susceptible of modification by volition. This theory he proceeds to apply to the explanation of several phænomena relating to the motions of the eyelids, pharynx, cardia, larynx, muscles of inspiration, sphincter ani, expulsors of the fæces and semen, to the tone of the muscular system generally, and to actions resulting from the passions. Lastly, he considers its application to various diseased states of the same functions, as manifested in cynic spasm, vomiting, asthma, tenesmus, strangury, crowing inspiration, convulsions, epilepsy, tetanus, hydrophobia, and paralysis.

Reference is made, in the course of the paper, to several drawings and diagrams, which, however, have not yet been supplied.

March 9, 1837.—A paper was read, entitled, "Researches on the

Tides. Seventh Series. On the Diurnal Inequality of the Height of the Tide, especially at Plymouth and at Sincapore: and on the Mean Level of the Sea." By the Rev. W. Whewell, A.M., F.R.S., Fellow of Trinity College, Cambridge.

The diurnal inequality which the author investigates in the present paper, is that by which the height of the morning tide differs from that of the evening of the same day; a difference which is often very considerable, and of great importance in practical navigation, naval officers having frequently found that the preservation or destruction of a ship depended on a correct knowledge of the amount of this variation. In the first section of the paper he treats of the diurnal inequality in the height of the tides at Plymouth, at which port good tide observations are regularly made at the Dock Yard; and these observations clearly indicate the existence of this inequality. As all the other inequalities of the tides have been found to follow the laws of the equilibrium theory, the author has endeavoured to trace the laws of the diurnal inequality by assuming a similar kind of correspondence with the same theory; and the results have confirmed, in the most striking manner, the correctness of that assumption. By taking the moon's declination four days anterior to the day of observation, the results of computation accorded, with great accuracy, with the observed heights of the tides: that is, the period employed was the fifth lunar transit preceding each tide.

In the second section, the observations made on the tides at Sincapore from August 1834 to August 1835, are discussed. A diurnal inequality was found to exist at that place, nearly agreeing in law and in amount with that at Plymouth; the only difference being that, instead of four days, it was found necessary to take the lunar declination a day and a half preceding the tide; or, more exactly, at the interpolated, or north lunar transit, which intervened between the second and third south transit preceding the tide. The diurnal inequality at Sincapore is of enormous magnitude, amounting in many cases to six feet of difference between the morning and evening tides; the whole rise of the mean tide being only seven feet at spring tides, and the difference between mean spring and neap tides not exceeding two feet.

In the third section, the author considers the diurnal inequalities at some other places, and the general law of its progress. The change which the *epoch*, (that is, the anterior period at which the moon's declination corresponds to the amount and direction of the inequality,) in particular, undergoes, is a subject of great interest. At Liverpool, the epoch is found to be about six days and a quarter; at Bristol, it is nearly six days; and at Leith, it is as much as twelve days. On the east coast of America, it appears to be *zero*. On the coasts of Spain, Portugal, and France, it is successively two, and three days; and on those of Cornwall and Devonshire, four days; thus observing a tolerably regular augmentation as it is traced along the line of coast from the shores of the Atlantic to the Firth of Forth, but travelling more slowly than the other inequalities.

In section fourth, the author treats of certain extreme cases of diurnal inequality; particularly those which produce the phænomenon of a single tide in the twenty-four hours: such as that noticed by Capt. Fitzroy at King George's Sound, on the south coast of New Holland; and that of Tonquin, referred by Newton to the interference of two tides arriving by different channels, but probably owing to the operation of the same law as that which gives rise to the diurnal inequality.

In section fifth, the author considers the subject of the mean height of the sea; that is, the height midway between low water and high water each day: and arrives at the result that it is very nearly constant.

March 16.— A paper was read, entitled, "On the Tides." By John William Lubbock, Esq., F.R.S., &c.

Since the author presented his last paper on the tides to the Society, his attention has been directed to ascertain the three following points: namely, 1st, Whether, from the discussion of the Liverpool observations with reference to a previous transit, these observations present the same kind of agreement with Bernoulli's theory as those of London: 2ndly, Whether, by taking into account a greater number of observations, the results given in his last paper remain sensibly unaltered: and 3rdly, Whether the *establishment* of the Port of London varies sensibly in different years; and whether the removal of the old London bridge has occasioned any difference. In order to elucidate these points, he procured the assistance of Mr. Jones and Mr. Russell to compute numerous tables; employing for that purpose a further sum of money placed at his disposal with this view by the British Association for the Advancement of Science. The results contained in the tables here presented, are all laid down in diagrams, on the same plan as those contained in his last paper, by which means they are much more readily understood. The author finds that the semi-menstrual correction for the interval at Liverpool presents the same agreement with observation as had been before noticed; while the form or law of the semi-menstrual correction for the height is also the same as that indicated by the observations; but in order to render the agreement complete it would be necessary to change the epoch, or to make a slight movement of the theory-curve in the diagrams. This remarkable difference also obtains in the London semi-menstrual correction for the height.

The calendar month inequality at Liverpool, considered as resulting implicitly from the corrections due to changes in the declinations of the luminaries, and in the sun's parallax, agrees generally with Bernoulli's theory, and with the results deduced from the London observations given in the author's last paper.

The author finds that the *Establishment* of the Port of London has been subject to changes even since the beginning of the present century, and he notices the difficulty of predicting the time of high water with accuracy unless these changes can be accounted for. He also cites a very ancient Tide Table, from which it would appear that for-

merly the time of high water at London was an hour later than it is at present.

The Society then adjourned over the Easter recess, to meet again on the 6th of April.

ROYAL IRISH ACADEMY.

October 24, 1836.—A paper was read, entitled "Contributions to the History of Pyroxylic Spirit, and the derived Combinations." By Robert J. Kane, M.D. M.R.I.A., Professor of Natural Philosophy in the Royal Dublin Society*.

A paper was also read, "On the laws of Reflexion from Metals." By James Mac Cullagh, M.R.I.A., Professor of Mathematics in the University of Dublin.

The author observes that the theory of the action of metals upon light is among the *desiderata* of physical optics, whatever information we possess upon this subject being derived from the experiments of Sir David Brewster. But, in the absence of a real theory, it is important that we should be able to represent the phænomena by means of empirical formulæ; and, accordingly, the author has endeavoured to obtain such formulæ by a method analogous to that which Fresnel employed in the case of total reflexion at the surface of a rarer medium, and which, as is well known, depends on a peculiar interpretation of the sign $\sqrt{-1}$. For the case of metallic reflexion, the author assumes that the velocity of propagation in the metal, or the reciprocal of the refractive index, is of the form

$$m(\cos\chi + \sqrt{-1}\sin\chi);$$

without attaching to this form any physical signification, but using it rather as a means of introducing two constants (for there must be two constants, m and χ, for each metal) into Fresnel's formulæ for ordinary reflexion, which contain only one constant, namely, the refractive index.

Then if i be the angle of incidence on the metal, and i' the angle of refraction, we have

$$\sin i' = m(\cos\chi + \sqrt{-1}\sin\chi)\sin i, \qquad (1)$$

and therefore we may put

$$\cos i' = m'(\cos\chi' - \sqrt{-1}\sin\chi')\cos i, \qquad (2)$$

if

$$m'^4\cos^4 i = 1 - 2m^2\cos 2\chi\sin^2 i + m^4\sin^4 i, \qquad (3)$$

and

$$\tan 2\chi' = \frac{m^2\sin 2\chi\sin^2 i}{1 - m^2\cos 2\chi\sin^2 i}. \qquad (4)$$

Now, first, if the incident light be polarized in the plane of re-

* This paper has appeared in our present volume, p. 45 *et seq.* See also the Proceedings of the British Association, also in Lond. and Edinb. Phil. Mag., vol. vii. p. 397.—Edit.

flexion, and if the preceding values of $\sin i'$, $\cos i'$, be substituted in Fresnel's expression

$$\frac{\sin(i-i')}{\sin(i+i')},$$

for the amplitude of the reflected vibration, the result may be reduced to the form

$$\alpha(\cos\delta - \sqrt{-1}\sin\delta), \tag{5}$$

if we put

$$\tan\psi = \frac{m}{m'}, \tag{6}$$

$$\tan\delta = \tan 2\psi \sin(\chi + \chi') \tag{7}$$

$$\alpha^2 = \frac{1 - \sin 2\psi \cos(\chi + \chi')}{1 + \sin 2\psi \cos(\chi + \chi')}. \tag{8}$$

Then according to the interpretation, before alluded to, of $\sqrt{-1}$ the angle δ will denote the *change of phase*, or the retardation of the reflected light; and α will be the amplitude of the reflected vibration, that of the incident vibration being unity. The values of m', χ', for any angle of incidence, are found by formulæ (3), (4), the quantities m, χ, being given for each metal. The angle χ' is very small, and may in general be neglected.

Secondly, when the incident light is polarized perpendicularly to the plane of reflexion, the expression

$$\frac{\tan(i-i')}{\tan(i+i')},$$

treated in the same manner, will become

$$\alpha'(\cos\delta' - \sqrt{-1}\sin\delta'), \tag{9}$$

if we make

$$\tan\psi' = m\,m', \tag{10}$$

$$\tan\delta' = \tan 2\psi' \sin(\chi - \chi'), \tag{11}$$

$$\alpha'^2 = \frac{1 - \sin 2\psi' \cos(\chi - \chi')}{1 + \sin 2\psi' \cos(\chi - \chi')}; \tag{12}$$

and here, as before, δ' will be the retardation of the reflected light, and α' the amplitude of its vibration.

The number $\text{M} = \frac{1}{m}$ may be called the *modulus*, and the angle χ the *characteristic* of the metal. The modulus is something less than the tangent of the angle which Sir David Brewster has called the maximum polarizing angle. After two reflexions at this angle a ray originally polarized in a plane inclined 45° to that of reflexion will again be plane polarized in a plane inclined at a certain angle φ (which is 17° for steel) to the plane of reflexion; and we must have

$$\tan\varphi = \frac{\alpha'^2}{\alpha^2}. \tag{13}$$

Also, at the maximum polarizing angle we must have

$$\delta' - \delta = 90°. \tag{14}$$

And these two conditions will enable us to determine the constants M and χ for any metal, when we know its maximum polarizing angle and the value of φ; both of which have been found for a great number of metals by Sir David Brewster. The following table is computed for steel, taking $M = 3\frac{1}{3}$, $\chi = 54°$.

i	δ	δ'	α^2	α'^2	$\frac{1}{2}(\alpha^2+\alpha'^2)$
0°	27°	27°	·526	·526	·526
30	23	31	·575	·475	·525
45	19	38	·638	·407	·522
60	13	54	·729	·308	·518
75	7	98	·850	·240	·545
85	2	152	·947	·491	·719
90	0	180	1·	1·	1·

The most remarkable thing in this table is the last column, which gives the intensity of the light reflected when common light is incident. The intensity *decreases* very slowly up to a large angle of incidence, (less than 75°,) and then increases up to 90°, where there is total reflexion. This singular fact, that the intensity decreases with the obliquity of incidence, was discovered by Mr. Potter, whose experiments extend as far as an incidence of 70°*. Whether the subsequent increase which appears from the table indicates a real phænomenon, or arises from an error in the empirical formulæ, cannot be determined without more experiments. It should be observed, however, that in these very oblique incidences Fresnel's formulæ for transparent media do not represent the actual phænomena for such media, a great quantity of the light being stopped, when the formulæ give a reflexion very nearly total.

The value of $\delta' - \delta$, or the difference of phase, increases from 0° to 180°. When a plane-polarized ray is twice reflected from a metal, it will still be plane-polarized if the sum of the values of $\delta' - \delta$ for the two angles of incidence be equal to 180°.

It appears from the formulæ that when the characteristic χ is very small, the value of δ' will continue very small up to the neighbourhood of the polarizing angle. It will pass through 90°, when $m\,m' = 1$; after which the change will be very rapid, and the value of δ' will soon rise to nearly 180°. This is exactly the phænomenon which Mr. Airy observed in the diamond†.

Another set of phænomena to which the author has applied his formulæ are those of the coloured rings formed between a glass lens and a metallic reflector; and he has thus been enabled to account for the singular appearances described by M. Arago in the *Mémoires d'Arcueil*, tom. iii., particularly the succession of changes

* A notice of Mr. Potter's experiments will be found in Phil. Mag. and Annals, vol. viii. p. 60.—Edit.

† Mr. Airy's paper, in which this phænomenon is described, appeared in Lond. and Edinb. Phil. Mag., vol. ii. p. 20.—Edit.

which are observed when common light is incident, the intrusion of a new ring, &c. But there is one curious appearance which he does not find described by any former author. It is this. Through the last twenty or thirty degrees of incidence the first dark ring, surrounding the central spot which is comparatively bright, remains constantly of the same magnitude; although the other rings, like Newton's rings formed between two glass lenses, dilate greatly with the obliquity of incidence. This appearance was observed at the same time by Professor Lloyd. The explanation is easy. It depends simply on this circumstance, (which is evident from the table,) that the angle $180° - \delta$, at these oblique incidences, is nearly proportional to $\cos i$.

As to the index of refraction in metals, the author conjectures that it is equal to $\frac{\mathrm{M}}{\cos \chi}$.

Rev. Robert Gage exhibited specimens of Coal and Ironstone, recently found in Rathlin Island, on the north coast of Ireland.

Nov. 30, 1836.—Sir William Betham exhibited to the Academy a specimen of the ancient brazen ring money, found in the county of Monaghan, and also a piece of cast iron, found with many others, in boxes, on board a vessel wrecked on the coast of Cork last summer. This vessel was bound to Africa, where it is stated the pieces in question pass for money. They are so similar in shape and size to the ancient specimens, that there can be no reasonable doubt of the identity of their uses; and thus the theory advanced in the paper referred to is strongly confirmed.

Sir William Betham also read an extract of a letter from a friend, in which it was stated, that gold rings, exactly formed like those found in the Irish bogs,—that is, of gold wire turned into the form of rings, but not united at the ends,—pass current at this moment as money in Nubia and Sennaar.

The Dean of St. Patrick's exhibited two bronze specimens of the first-mentioned articles found in Italy, one of which was encrusted with crystals of carbonate of lime.

The following papers were read: 1. "On the Affinity of the Hiberno-Celtic and Phœnician Languages." By Sir William Betham, M.R.I.A., Secretary of Foreign Correspondence. An abstract of this paper appears in the "Proceedings" of the Academy, No. 1.

2. "On the Propagation of Light in Uncrystallized Media." By the Rev. H. Lloyd, F.R.S., M.R.I.A., Professor of Natural Philosophy in the University of Dublin.

The objects of the author have been—1. to simplify and to develop that part of M. Cauchy's theory which relates to the propagation of light in an æthereal medium of uniform density; 2. to extend the same theory to the case of the æther inclosed in uncrystallized substances, taking into account the action of the material molecules.

Some of the simplifications adopted in the first part of these in-

quiries suggest themselves naturally. Thus the *axes of symmetry* of the medium are taken as the axes of coordinates, and the direction of propagation is assumed to coincide with one of these axes. By these suppositions the differential equations of motion are reduced to a very simple form; and it is manifest that the assumptions themselves involve no real limitation of the problem. The well-known expressions for the component displacements are deduced by the integration of these equations. The following is that in the direction of the axis of x:

$$\xi = a \cos (ut - kz + \alpha);$$

in which

$$u = \frac{2\pi}{\tau}, \; k = \frac{2\pi}{\lambda},$$

τ being the period of vibration, and λ the length of the wave. These quantities are connected by a relation given by the method of integration.

The preceding formula, however, is not the most general form of the expression for the displacement. It is found that in certain cases the integral becomes

$$\xi = ae^{-hz} \cos (ut - gz + \alpha).$$

From this expression it follows that the amplitude of the displacement, and therefore the intensity of the light, decreases in geometrical progression, as the distance increases in arithmetical progression; and as the constant h is in general a function of u, or of the colour, the differently coloured rays will be *differently absorbed.* The complete value of ξ being the sum of a series of terms similar to the preceding, it is manifest that we have here a satisfactory account of the apparently irregular distribution of light in the absorbed spectrum. To explain the absolute deficiency of the light at certain points, it is only necessary to admit that the function h varies in certain cases *rapidly* with moderate changes in u, and becomes *very great* for certain definite values of that quantity.

The preceding integral has been already obtained by M. Cauchy, in a valuable memoir recently printed in lithograph. The method employed by the author seems, however, to be fundamentally different from that of M. Cauchy; and in fact he was led to this form of the integral by other considerations before he was aware that he had been preceded in the deduction.

The remainder of the present communication is taken up with the discussion of the relation between the coefficients u and k, which expresses the law of dispersion. Following M. Cauchy*, the author has transformed this relation by converting the triple sums into triple integrals; and he has found that, by applying this transformation at an earlier stage of the investigation, the resulting relation is deduced with great simplicity.

* *Nouveaux Exercices de Mathématiques* Livraison 7^{me}.

The relation between u and k, for the vibrations in the plane of the wave, has already yielded to M. Cauchy the probable result, that the molecules of the æther repel one another according to the inverse fourth power of the distance. When this law of force is substituted in the corresponding relation for the *normal* vibration, the author finds that the resulting value of $\frac{u}{k}$, or of the velocity of propagation, is infinite; so that the normal disturbance is propagated *instantaneously*, and gives rise to no wave. Thus the hypothesis of transversal vibrations seems to be established on theoretical grounds.

The author finally gives reasons for concluding that the theory, in its present form, is insufficient to explain the phænomena of light in bodies; and that it becomes necessary in this case to take into account the action of the material molecules. This extension of the theory will be given in a future communication.

3. "On the Composition of Thebaine." By Robert J. Kane, M.D., M.R.I.A., Professor of Natural Philosophy in the Royal Dublin Society.

The author gave an account of the analysis of the vegetable alkaloid thebaine (paramorphine) which had been discovered in opium, and of which the analysis by Pelletier and Couerbe gave discordant results. With a specimen which had been prepared by Apothecary Merck of Darmstadt, Dr. Kane obtained the following formula $C_{25}\,N_2\,H_{28}\,O_3$, (Berzelian atoms,) and giving the per cent. composition:

25 Carbon	= 74·57 =	1910·925
2 Azote	= 6·89 —	177·036
28 Hydrogen	= 6·83 —	174·714
3 Oxygen	= 11·71 —	300·000
	100·00	2562·675

Owing to the circumstance of the salts of this base with the mineral acids being uncrystallizable, the atomic weight obtained by analysis could not be synthetically confirmed.

Professor Kane read likewise an extract of a letter from Professor Liebig, of Giessen, communicating some new results of chemical analysis.

It was resolved, on the recommendation of Council, that the "Proceedings of the Royal Irish Academy" be printed every month during its sittings, for the use of its members. The "Proceedings" to be under the management of the Council, and to contain,—1. Abstracts of the larger papers read to the Academy. 2. Minor communications, not intended for the Transactions, printed more at length. 3. Notices of the election of members, of presents received, and of all other matters of general interest transacted at the meetings of the Academy.

GEOLOGICAL SOCIETY.

[*The Address of the President,* Charles Lyell, *jun., Esq., at the Anniversary,* 1837.—Continued from p. 316.]

We are indebted to Mr. Austen for a description of the South of Devonshire between the river Ex and Berry Head, and between the coast and Dartmoor, a district consisting of transition rocks, new red sandstone, greenstone, and trap. His speculations on the origin of the different formations and the causes which gave rise to the existing features in the physical geography of the country display much talent and are full of instruction*.

The structure of Devonshire has also furnished a fertile field of inquiry to Messrs. Sedgwick and Murchison since our last Anniversary. They have attempted the difficult task of establishing a classification of the older rocks so largely developed in that county. In every geological map hitherto published of Devonshire, all the stratified deposits of higher antiquity than the new red sandstone had been represented by one common colour, the limestones being all included as integral parts of one great formation called graywacké†. But these gentlemen, after examining this region, announced at Bristol to the geologists assembled at the Meeting of the British Association, that the great mass termed graywacké, and previously undivided, comprised in it several formations of great thickness, ranging in age from the Cambrian system of Professor Sedgwick up to the true carboniferous series inclusive. The first groups mentioned by them in ascending order are the Cambrian and Lower Silurian, which great mass contains many distinct courses of limestone; and is separable into several formations, distinguishable from each other by stratigraphical position and by lithological and zoological characters.

There appears, however, to be a great hiatus in the succession of rocks in Devonshire, as compared to South Wales, there being no traces of the upper Silurian strata, nor of the old red sandstone, nor even of the mountain limestone in its ordinary aspect. On the contrary, the next group met with in ascending order, is a culmiferous series, the base of which distinctly reposes upon the above-mentioned ancient rocks. This culmiferous deposit, far from appearing as a mere band, or at detached points, occupies about one third of the large county of Devon, and a considerable adjacent part of Cornwall; its southern boundary ranging from Exeter on the east, by Launceston, to St. Gennis in Cornwall on the west; its northern frontier running by Barnstaple and South Moulton to near Wellington in Somersetshire. These culmiferous beds are shown to

* [An abstract of Mr. Austen's paper appeared in Lond. and Edinb. Phil. Mag. vol. ix. p. 495.—Edit.]

† The Abstract of the Report of Messrs. Sedgwick and Murchison, published with a section in the Athenæum, August, 1836, and in other scientific journals, is the same as that written for insertion in the Proceedings of the Association. From that document, and from a written explanation of their views, which I obtained from the authors, the present observations are deduced.

contain thick beds of limestone, entirely dissimilar in structure and fossil contents from any limestones of the underlying "grauwacke," in which they had previously been merged. The culm measures consist of grit, sandstone, shale and limestone; and these rocks, it is said, are never affected by a slaty cleavage like the lower Silurian and Cambrian rocks on which they rest. From this character, as well as from their prevailing mineralogical structure and imbedded fossil plants, the authors regard the culmiferous formation of Devon as perfectly identical in age with other coal-fields, and as more particularly analogous to the culm-bearing strata of Pembrokeshire; a part of which also once passed for "grauwacke," but Mr. Murchison has recently shown that it belongs to the South Welsh coal-field, which is known by all geologists to rest upon mountain limestone.

Thus referred to the age of our ordinary coal, these strata of North Devon are further proved to lie in a great trough, their southern edges being turned up against the granite of Dartmoor, where they acquire, in contact with the granite, when traversed by elvan dykes, many characters of the metamorphic rocks, or those commonly termed primary. The phænomena of interference and alteration at the junction are such as to give a comparatively modern date for the eruption of the Dartmoor granite, and to explain why so much difficulty and ambiguity has prevailed in determining the age of some of the altered culm beds.

Among other points which this survey of Professor Sedgwick and Mr. Murchison has settled, so far as Devon is concerned, is one of the highest theoretical interest, and on which for more than two years the Society has been anxiously desiring more accurate information; I allude to the true stratigraphical position of certain shales near Bideford in North Devon, containing fossil plants of the same species as those which are found abundantly in the coal. I may first remind you that a discussion had previously arisen respecting the alleged discovery by Mr. Weaver of anthracite, with the usual carboniferous plants, in the graywacké or transition rocks of Ireland*. Notwithstanding the value justly attached to the opinion of so experienced and long-practised an observer, your Council hesitated to print his statement, and requested him to reexamine the ground. At the same time Mr. Griffith, to whom we are looking for the publication of a Geological Map of Ireland, had come to a different conclusion, and Mr. Weaver having been induced to repeat his observations, became convinced that he was in error, and has since studiously availed himself of every opportunity of announcing this change in his views.

You are aware that as yet in the British islands, scarcely any vegetable impressions have been met with in rocks more ancient than the carboniferous strata above the old red sandstone, so that we know not what species of plants belong to the graywacké or transition group. We can only presume from analogy that since the shells, corals, and other

* Phil. Mag. and Annals, vol. viii. p. 147.

organic remains of that ancient group differ from those found above the old red sandstone, the plants also, if ever discovered, will differ as greatly. Considerable surprise was therefore excited when, during the Presidentship of my predecessor in this chair, a letter was read, addressed to him from Mr. De la Beche, stating that he had found, near Bideford in North Devon, many well known coal plants in the lower greywacke, or far down in the transition series*. Such of the plants as were determinable had been identified by Professor Lindley with species characteristic of the true coal measures, and which had never been found elsewhere below the coal. The anomaly, therefore, in the supposed position of these fossils was so great, that between the ordinary geological site of such remains, and that in which they were here inferred to present themselves, there would be interposed if the series were complete the whole of the old red sandstone, and at least the two upper formations of the Silurian system. When this point was considered, I expressed to the Society my opinion, in common with Mr. Murchison, as to the insufficiency of the proofs relied on by our Foreign Secretary, and we felt that we had a right to call for more conclusive evidence. The simple fact of shales having been found charged with true coal plants, raised so strong a presumption in favour of their belonging to the regular carboniferous series, that the burthen of proof rested with him who wished to assign to them either a higher or lower position. Our scepticism was regarded by Mr. Greenough as implying too marked a bias for a preconceived theory, and this he afterwards hinted in his Anniversary Address†. I may affirm, however, that in the first place it implied on my part no distrust of Mr. De la Beche's skill or experience in geological surveying, and that had Professor Sedgwick and Mr. Murchison advanced a similar opinion on analogous proofs, I should equally have withheld my assent. Suppose, for example, they had announced to us that they had found fossil fruits and leaves identical with those of Sheppey in strata of the age of the white chalk with flints. I should have demanded from them, in corroboration, the most clear, unequivocal, and overwhelming evidence. If it were a region of disturbed and vertical strata, I should expect them first to have resorted in vain to every hypothesis of inverted stratification with a view of explaining away such an exception to the general rule.

I might perhaps be told that we are unacquainted with the flora of the upper cretaceous period, and I admit that we are as ignorant of it as of that which belonged to the transition period; but when we consider the contrast of the shells and other fossils of the chalk and London clay, we naturally anticipate that if plants are ever found of the precise age of our chalk with flints, they will not prove to be of the same species as those of the Sheppey clay. There is a like presumption from analogy against the conclusion that the same vegetation continued to flourish on the earth from the period of the lower greywacke to that of the coal, because we know that

* Lond. and Edinb. Phil. Mag., vol. vi. p. 67. † *Ibid.*, vol. vii. p. 152.

in the course of the intervening epochs the testacea, zoophytes, fish, and other classes of organic beings were several times changed.

In regard to the proofs relied on by Mr. De la Beche, I should observe that he never attempted to show that the plant-bearing shales at Bideford were interstratified with rocks charged with shells or other fossils known to belong to rocks older than the old red sandstone.

Since writing the above sketch of the different views recently published of the structure of Devonshire, I have received a letter from Mr. De la Beche, from which I am happy to learn, that it is his intention before concluding his report on the Ordnance Map of Devon, to reexamine Devonshire. He is far, he says, from pretending that his first views were perfect, and if he finds reason to modify any of them, he shall not hesitate to announce the change of opinion. In the mean time he no longer contends that the culmiferous strata are referable to the lower graywacké, and considers the point of difference to lie within a narrower compass, namely, whether the culm beds are to be considered as upper graywacké or coal. This question, on which he is not yet satisfied, evidently appears to him of much less theoretical importance than, I confess, it does to me. It is fair, however, that I should state the arguments which influence his mind. If the plants, he says, found at Bideford in the culmiferous series should belong to strata more ancient than the old red sandstone the fact would not stand alone, for he has lately received a letter from M. Elie de Beaumont, detailing analogous phænomena in Brittany. It is stated that the greywacke there closely corresponds in general character with that of Devon, the upper part like the Devonian series containing anthracite. With this anthracite or culm are found at Montrelais, Chatelaison, and other places, fossil plants, the greater part of which are identical with those in the coal measures; but there are others which have not hitherto been detected in the latter rock. Patches of true coal measures rest in unconformable position upon these upper graywacké beds of Brittany. Now I regret that I have not seen any printed account of the geology of this part of France; for until we learn whether the plants in question are associated with true Silurian fossils, the testimony is quite incomplete. We know not, for instance, whether the plant-bearing series in question is old red sandstone or a Silurian formation, or whether it is a lower part of the true carboniferous system of which the strata had been disturbed before a higher portion was superimposed.

Similar remarks hold in regard to the observations made by M. Virlet in the Dictionnaire d'Hist. Naturelle, where in his late article " De l'Origine des Combustibles Minéraux," he speaks of certain carboniferous deposits of Ireland, (those alluded to by Mr. Weaver before mentioned,) as well as others examined by M. Voltz in the Black Forest, also the culm beds of Brittany, and those of the department of La Sarthe, as all belonging in age to the newest transition formations, " *terrains de transition les plus récens*."

Mr. De la Beche alludes to another discovery of coal plants

implying as great an anomaly as that which he had imagined to occur in Devonshire, and by which he was himself once led into error during an Alpine excursion, about eighteen years since, when he met with coal plants in the schists of the Col de Balme, in Switzerland. He then inferred that the beds belonged to the true coal measures, but M. Elie de Beaumont afterwards proved them to be lias; that is to say, he identified them with other rocks not far distant in the Alps, which were shown to be lias by containing Belemnites and other fossils. Mr. De la Beche was at first sceptical on the point, but after revisiting the Alps, he came round to the same opinion. Having therefore been in one instance misled by relying on the fossil vegetables of the coal as affording a good chronological test, he naturally attached but small value to the same testimony as a criterion of the age of another set of rocks in Devonshire. Now you will easily understand that a geologist, who is once persuaded that the same plants flourished in European latitudes from the period of the true coal to that of the lias, will be ready to concede without difficulty the probable existence of the same plants at an era long antecedent to the coal. We know that between the deposition of the coal and the lias there were successive revolutions in the races of animals which inhabited the waters, the zoophytes, mollusca, fish, and, as far as we know them, the reptiles having been changed again and again; so that the fossils of the mountain limestone differ from those of the magnesian limestone or zechstein, these again from the organic remains of the muschelkalk, and these last from those of the lias. If we are to believe that the same plants survived on the land, while such fluctuations in animal life occurred in the waters, why should we not imagine the longevity of the same species to have been still greater, so that they began to exist even before the deposition of the old red sandstone? But let me remind you that botanists have been led to very different conclusions respecting the laws governing the distribution of fossil vegetables from the study of undisturbed districts. You are not ignorant that the strata of the Alps are involved in extreme confusion and complexity, mountain masses having been completely overturned and twisted, so that the same set of strata have been found at the top and bottom of the same section separated by several thousand feet of beds belonging to an older formation. So obscure is the order of position in Alpine geology, that the cretaceous and greensand series have been classed by experienced geologists as more ancient than the oolite, under which, in point of fact, they occasionally lie.

Professor Studer, in his work on the Bernese Highlands, after years of personal investigation, has published a map in which he has given a coloured ground plan without venturing to commit himself by sections, or a table of the regular order of superposition.

After devoting a summer to the investigation of the same portion of Switzerland, with the advantage of Mr. Studer's map and work, I was unable to satisfy myself that I had found a key to the classification or superposition of the formations, so enormous is the scale

on which they have been deranged. I collected fossil plants on the Col de Balme, but I have not examined the precise localities further to the west appealed to by M. de Beaumont. I am far, therefore, from denying his facts or inferences, hoping at some future period more carefully to inquire into the evidence on the spot. No one, I am aware, is more desirous that others should visit the southern Alps and verify or criticise his facts than M. de Beaumont. Meanwhile I am reminded of an expression of our mutual friend M. von Buch. When I related to him some geological phænomena which surprised him; "I believe it," he said, "because you have seen it, but had I only seen it myself, I should not have believed it."

But to conclude, and to recall your attention to the structure of Devonshire, you will perceive that Mr. Murchison and Professor Sedgwick have endeavoured, and I think successfully, to work a great reform in the classification of the ancient rocks of that country, by applying to them the arrangement which they had previously made for the deposits termed by them Cambrian and Lower Silurian in Wales and the adjoining parts of England. According to their survey and sections the coal plants of Bideford, so far from constituting any anomaly, so far from affording any objection to the doctrine that particular species of fossil plants are good tests of the relative age of rocks, do in reality from the place which they occupy, confirm that doctrine; for the culmiferous rocks distinctly overlie the so-called grauwacké, and are not referable to any of the well-defined and normal types, which compose the old Red Sandstone and Silurian System.

I shall now pass on to the consideration of other memoirs on English Geology. The limestone which the Germans call muschelkalk, and the numerous fossils which are peculiar to it, have not yet been detected in England in any part of that great series of beds which intervene between the lias and the coal. In those parts of Germany where it occurs, it divides the beds of red marl and sandstone which occupy that great interval into two divisions, the upper of which is called keuper, and the lower bunter sandstein. In the absence of the muschelkalk in this country, it has been impossible for us to separate our new red sandstone into two well-defined masses; but Dr. Buckland considers that certain portions of the upper beds in Warwickshire and elsewhere may be identified with the keuper by their mineral character, and near Warwick by the remains of a Saurian, which he believes to be of the genus Phytosaurus, a genus characteristic of the keuper of Wirtemberg.

An examination in the South-east of England of the strata usually termed plastic clay, has led Mr. John Morris to offer several new, and as they appear to me, judicious suggestions in regard to the classification of these beds. It is well known that wherever the tertiary strata are seen in immediate contact with the chalk, they consist of alternations of sand, clay, and pebbles, and in some few places a calcareous rock,—all these varying greatly in their thickness, and in their order of succession in different places. Mr. Morris divides those of Woolwich into two parts, and states

that the upper is characterized by a mixture of marine and freshwater shells, the freshwater genera being Cyrena, Neritina, Melanopsis, and Planorbis. The lower division contains exclusively marine shells. The author refers this intermixture to the influx of a river into the sea, in which the London clay was formed. Mr. Morris considers the Bognor strata, which rest immediately upon chalk, as the equivalents of the lower Woolwich deposit, observing that the shells agree with those of the London clay. These remarks seem to confirm the conclusion to which he had been previously led by the grand section at Alum Bay in the Isle of Wight, namely, that the beds usually styled plastic and London clays belong to one zoological period.

MINERAL VEINS.

Your attention has been called to the origin of mineral veins by Mr. Fox, who has endeavoured to explain why so large a proportion of the metalliferous veins in England and other parts of the world should have an east and west direction. He supposes fissures filled with water, containing sulphurets and muriates of copper, tin, iron, and zinc in solution, through which currents of voltaic electricity are transmitted. The metals separated from their solvents by this action are deposited in the veins, and most abundantly in veins running at right angles to the direction of the earth's magnetism; for as the magnetic currents of the earth pass from north to south, they cause those of electricity to move east and west, although considerable deviations from this direction must be occasioned in the course of geological epochs by variations in the magnetic meridian*.

Since Mr. Fox first ascertained the existence of electric currents in some of the metalliferous veins in Cornwall†, Mr. Henwood has made many experiments on the same subject, together with observations on the distribution of metallic and earthy minerals in veins. He considers the results obtained by him to be in a great degree opposed to the theory of Mr. Fox‡.

Mr. Fox conceives the fissures in which metalliferous substances occur, to have been at first small and narrow, and to have increased gradually in their dimensions. This doctrine has also been propounded in a work with which you are probably familiar, and from which I have derived much instruction, I mean M. Fournet's Essay on Metalliferous Deposits. This Essay was originally included in the 3rd volume of M. Burat's continuation of D'Aubuisson's Treatise on Geology (1835), but it is now published separately, and gives the clearest general view which I have seen of the application of geological theories to phænomena observed in mining. It is written

* [Mr. Fox's paper was noticed in Lond. and Edinb. Phil. Mag., vol. ix. p. 387.—Edit.]

† Phil. Trans. 1830, p. 399.

‡ See Mining Journal, Supplement 9. p. 34, December 1836, and Annals of Electricity, No. 2. vol. i. on Electric Currents, &c. by W. T. Henwood, Esq.

by one who has acquired much practical knowledge as a miner, and who is well versed in chemistry and mineralogy*.

Werner, when he published his justly celebrated Essay on Mineral Veins, had come to the conclusion that the same rent, after being wholly or partially filled, has sometimes been reopened; and M. Fournet has endeavoured more fully to explain the successive dilatation of the same veins at distinct periods. He has given examples in mines worked under his direction in Auvergne, in which the sulphurets of iron, copper, lead, and zinc, besides quartz, barytes, and other minerals, seem evidently to have been introduced at different periods by chemical action accompanied by new fractures and dislocations of the rocks, and the widening of preexisting fissures†.

You will find in M. Fournet's treatise a copious analysis of a great variety of books on mining, besides a detail of facts which have fallen under his own observation. He has described first those veins which are decidedly connected with rents produced in rocks by mechanical movements, and which are supposed to have been chiefly filled from below by sublimation, more or less obviously connected with volcanic action. He afterwards passes on to the consideration of those masses which have been called stockwerks by the Germans, which are imagined by some to have their origin in the contraction of granite, porphyry, and other rocks as they cooled, numerous rents being then formed, in which metallic particles were concentrated. In treating the subject in this order the author appears to me to have followed the most philosophical course, beginning with cases of undoubted rents of mechanical origin filled with minerals and metals introduced by sublimation, and then carrying with him as far as possible the light derived from these sources to dissipate a part of the obscurity in which all theories respecting the nature of Plutonic rocks and their minerals must, I fear, be for ever involved. Much will still remain unexplained; but those who proceed in an opposite direction often throw doubt and confusion upon the simplest phænomena, as has sometimes happened in an analogous case, when geologists have begun with the examination of granite and granite veins, and have then endeavoured to apply the ideas derived from this study to the trap rocks and volcanic dykes.

Among the most interesting conclusions deduced by M. Fournet from his examination of the mining districts of Europe, I may mention the modern periods at which the precious metals appear to have entered into some veins: thus, to select a single example, some veins of silver of Joachimsthal in Bohemia are proved to have originated in the tertiary period‡.

FOREIGN GEOLOGY.

Among the researches into the geology of foreign countries in which our members have been recently engaged, I have great

* Etudes sur les Dépots Métallifères, par M. I. Fournet.
† See "Etudes," &c. Section 3.
‡ See "Etudes," &c. Section 2.

pleasure in alluding to the labours of Mr. H. E. Strickland and Mr. Hamilton in Asia Minor*. These gentlemen first examined the neighbourhood of Constantinople, and found on both sides of the Thracian Bosphorus an ancient group of fossiliferous strata, consisting of schist, sandstone, and limestone. From the character of the fossils it is inferred that these rocks may probably be the equivalents of the upper transition or Silurian strata of England. The shells belong to the brachiopodous genera Spirifer, Producta, and Terebratula, with which the remains of corals and Crinoidea were associated, and fragments of a Trilobite.

The rarity of any fossiliferous deposits of higher antiquity than the old red sandstone in any of the countries bordering the Mediterranean, or indeed to the south of the Alps and Pyrenees, lends considerable interest to this observation. In their way through France, our travellers examined the well-known region of extinct volcanos in Auvergne, and afterwards found a counterpart to it in the Catacecaumene, a district in Asia known by that name in the time of Strabo, from its burnt and arid appearance. Some of the volcanos in Asia are of very modern appearance, although no notice of their eruptions falls within the limits of history or tradition. The volcanic hills rise partly through lacustrine limestone in the Valley of the Hermus, and partly cover the slope of the schistose hills which bound it to the south. There are about thirty older cones, worn by time, and of which the craters are effaced or only marked by a slight depression; and three newer cones, which preserve their characters unaltered, the craters being perfectly defined and the streams of lava still black, rugged, and barren. Here, as in the country of corresponding structure in France, we find streams of lava following the course of existing valleys, and yet frequently cut through by rivers. We find also a tertiary freshwater formation, sometimes resembling chalk with flints, like that of Aurillac in France, and forming detached hills capped with basalt, while more modern lavas have flowed at the base of the same hills. The extent of this analogy will be best appreciated by those who compare Mr. Strickland's drawings with Mr. Poulett Scrope's masterly illustrations of the French volcanic region.

The countries watered by the rivers Meander and Cayster are described as having a simple geological structure. There are granitic rocks, with saccharine marble, there are also hippurite limestone and schist, and tertiary deposits unconformable to these, besides igneous rocks of various ages. The tertiary formations are chiefly lacustrine, and occur in nearly every large valley. They are composed of horizontal beds of calcareous marl and white limestone, in which are layers and nodules of flint; they also consist of sandstone, sand, and gravel.

The only representative of the secondary rocks of Europe is termed by Mr. Strickland "hippurite limestone", which appears to

[* An abstract of Mr. Strickland's paper has appeared in our present volume, p. 68.—EDIT.]

be very sterile in fossils. In this respect and in its other characters it agrees with that great calcareous formation described by MM. Boblaye and Virlet in their splendid work on the Geology of the Morea*. According to these French geologists, three quarters of the Peloponnesus are occupied by a compact limestone several thousand feet thick, in which they could discover scarcely any organic remains, except a few hippurites and nummulites, but which is supposed to be the equivalent of our chalk and oolites. Nothing, they say, can be more monotonous in character than this calcareous mass in the South of Europe, which appears to represent the larger part of our upper secondary formations of the North, where the rocks are so varied in lithological aspect and so distinguishable from each other by their well-preserved fossils.

Ancient fossiliferous strata resembling those of the neighbourhood of Constantinople are said to be largely developed in the Balkan, a mountain chain of which we may soon expect to receive information from the pen of M. Ami Boué. That indefatigable geologist has already explored a large part of Servia, a country of whose physical and moral condition we are perhaps more ignorant than of any other in Europe, and he is rapidly extending his survey over various parts of the Turkish empire, to the examination of which he proposes to devote several years. Meanwhile our late Secretary, Mr. Hamilton, is continuing, with great zeal, his investigation of the borders of the Black Sea and other parts of Asiatic Turkey.

In a paper on the structure of part of the Cotentin near Cherbourg, the Rev. W. B. Clarke describes that country as consisting of hills or ridges of quartz rock alternating with valleys of slate occasionally associated with syenite and greenstone, which appear to be of posterior origin. A curious fact is mentioned: the quartz rock splits naturally into irregular masses, which have, nevertheless, some angles of fixed dimensions, namely, 103°, 64°, and 83°. Fragments of a green variety of schist exhibit the same angles under the same circumstances of position, proving that similar causes had acted on the two formations *en masse*, the same sets of joints, lines of stratification, and cleavage being found in both. Besides these facts, which are illustrated by diagrams, the author mentions others calculated to throw light on the cleavage and jointed structure of rocks.

PROOFS OF MODERN ELEVATION AND SUBSIDENCE.

Under this head I shall first consider several notices of beds of gravel, sand, clay, and marl, containing recent marine shells, which have been observed in various parts of Great Britain, a subject very frequently brought before our notice of late years. Deposits of this kind have been found by Dr. Scouler in the vicinity of Dublin, where they rise to the height of 80, and in some places of even 200

* Paris, 1833, in folio. It is to be regretted that this work cannot be procured separately from other folios containing the scientific information collected during the French expedition to the Morea.

feet above the level of the sea. Besides marine shells of existing species, he has ascertained that some of the lower beds of this formation contain bones of the extinct Irish elk, by which we learn that this quadruped, although belonging to a comparatively modern period, and found in peat-mosses, had nevertheless begun to inhabit this part of the world at a period anterior to some of the last changes in the position of land and sea, changes which are proved by the upraised shelly beds just alluded to. Now Professor Nilsson of Lund in Sweden, although ignorant of these facts, had remarked to me that some great alteration must have occurred in the shape and extent of dry land and sea in Great Britain and the surrounding parts subsequently to the time when the Irish elk existed, otherwise so many entire skeletons of so large an herbivorous quadruped as the *Cervus megaceros*, would not have been found in so small an island as the Isle of Man. That island may at no remote geological period have been united to the main land, and may have since been separated from it by subsidences, on a scale equal to the elevations of which there is such clear evidence in Ireland and elsewhere.

Changes in the relative level of land and water, in the estuary of the Clyde, are indicated by facts described in another paper by Mr. Smith of Jordan Hill, near Glasgow. Superficial deposits, in which a great number of marine shells of recent species are imbedded, are found on the banks of the Clyde below Glasgow, at the height of 30 or 40 feet above the sea. I had myself an opportunity of verifying during the last summer several of these observations of Mr. Smith, and found equally clear proofs that the Island of Arran had participated in the upward movement, so that a circle of inland cliffs may be traced all round that island, between the base of which and the present high-water mark a raised beach occurs, and in some places beds of marine marls, formed of recent shells, as in the bay of Lamlash. Mr. Smith has also traced sea-worn terraces on each side of the Clyde below Dumbarton and between the Cloch Lighthouse and Largs.

We are indebted to Sir Philip Egerton for some new details respecting the shelly gravel of Cheshire, of which he had previously treated; and to Mr. Murchison and Professor Sedgwick for a joint paper on "a raised beach in Barnstaple Bay on the north-west coast of Devonshire." This beach puts on for several miles where it is best exposed, the form of a horizontal under terrace resting upon an indented and irregular surface of the older formations. It presents a cliff towards the sea, in which beds of calcareous grit, sandstone, and shingle are seen perfectly stratified. The bottom of the deposit is chiefly composed of indurated shingles resting on the ledges of the older rocks, and filling up their inequalities. Through the whole cliff, but especially in the indurated grits, shells are abundantly dispersed, identical in species with those now living on the coast, and well preserved, though sometimes waterworn.

The authors point out that these beds cannot have been formed by accumulations of blown sand. They demonstrate an elevation of the coast during the modern period; and there are phænomena both on the north and south coasts of Devonshire and Cornwall, which

afford proofs of modern changes in the level of the land, both of upheaval and depression. The raised beach of Hope's Nose, correctly described by Mr. Austen, is the most striking instance in South Devon.

The quantity of rise of land in the modern period is from ten to forty feet in South Devon and Cornwall, nearly seventy feet in North Devon, while in Lancashire, Cheshire, and Shropshire there are marine deposits with recent shells at the height of from 300 to 500 feet above the sea.

It is natural to inquire what changes the surface of the dry land in England may have undergone during the occurrence of such upward and downward movements. Perhaps some observations lately made by Mr. Bowerbank in the south of the Isle of Wight may elucidate this point. He has given us an account of a bed of chalky detritus, containing recent land shells, at Gore Cliff. This bed is ten feet thick, and rests immediately upon chalk marl. Many of the shells, which are plentifully scattered through it, retain their colour. As the deposit ranges to the foot of St. Catherine's Down, it is possible that the waste and denudation of that chalk hill may have supplied the materials. I have lately seen similar detritus resting on the chalk with flints, and arranged in numerous thin layers in the section exposed in cutting the railroad at Winchester, where a black layer of peaty earth and carbonized wood intersects thin layers of white chalk rubble, from twenty to thirty feet thick. Such appearances are, in fact, very general in chalk districts; a bed of flints not waterworn occurring on the highest downs, while fragmentary chalk, often inclosing land shells, occurs on their slopes and at lower levels. Violent rains have been known even of late years to tear off the turfy covering from certain points near Lewes, and to wash away flints and chalky mud, and leave them in the hollow combs or flanks of the hills. This action of the elements would be most powerful at periods when the chalk first emerged from the sea, or whenever it assumed in the course of subterranean disturbances a new position or physical outline.

We must, I think, infer from the occurrence of certain recent marine shells and shingle in the bottom of what has been termed the elephant-bed at Brighton, that the chalk in the South-east of England has undergone some movements of a modern date, the land having subsided there to the depth of fifty or sixty feet, and having been subsequently raised up again to a level somewhat higher than its original position*.

If it should appear upon careful research that the land shells found in terrestrial alluviums covering the chalk are almost universally of recent species, I should not conclude that the emergence of the chalk hills from the sea had generally occurred at a very modern period, but merely that these hills had been modified in shape in recent times, and that during that modification alluviums of older date had been washed away, or the land shells which they may once

* See Principles of Geology, 4th edit., vol. iv. p. 274.

have contained have decomposed and disappeared. In regard to the great numbers of these shells preserved throughout the bed at Gore Cliff, and in many other places even at greater depths, it will not seem surprising to those who have observed the number of dead land shells which are strewed over the surface of the chalk downs, or lie concealed in the green turf in numbers almost as countless as the blades of grass. If the slightest wash of water should pass over such a soil, it must float off myriads of these shells, and they would immediately be involved in that white cream-coloured mud which descends from wasting hills of chalk after heavy rains. Land shells so buried may retain their colour for indefinite periods, as is shown by the state of species in the loess of the Rhine, and even in tertiary strata of much higher antiquity.

While a variety of geological monuments are annually discovered which attest modern alterations in the level of the land, it is important to remark that new testimony is also daily obtained of the rising and sinking of land in our own times. I discussed at some length, in my last Anniversary Address, the evidence for and against the upheaval of the coast of Chili during the earthquake of 1822, a controverted point to which our attention has lately been again recalled. I may remark, however, that since we have ascertained the fact of a rise of three, five, and even ten feet in parts of the same country in 1835, so distinctly attested by Captain Fitzroy, all doubts entertained as to the permanent effects of a preceding convulsion are comparatively of small interest. Don Mariano Rivero dissents from the opinion that a change of level occurred at Valparaiso in 1822, and Colonel Walpole, after seeing the ground and conversing with persons who were on the spot in 1822, and who still reside there, also considers the statement of a rise to have been inaccurate. On the other hand Mr. Caldcleugh, who was formerly sceptical on the same point, has now come round to the opinion of Mrs. Callcott (Maria Graham), and believes that an elevation of land did take place.

Mr. Darwin, whose opportunities of investigation both in Chili and other parts of South America have been so extensive, thinks it quite certain that the land was upheaved two or three feet during the earthquake of 1822, and he met with none of the inhabitants who doubted the change of level. He states that the rise of land, even in the bay of Valparaiso, was far from being uniform, for a part of a fort not formerly visible from a certain spot has, subsequently to the earthquake, fallen within the line of vision. The most unequivocal proof of a recent rise is drawn from the acorn-shells, *Balanidæ*, found adhering to the rock above the reach of the highest tides. These were observed by Mr. Darwin sixty miles south of Valparaiso, and at Quintero, a few miles to the north of it; but his friend Mr. Alison detected them on a projecting point of rock at Valparaiso itself. The attached shells were there seen at the height of fourteen feet above high-water mark, and were only exposed upon the removal of the dung of birds, by which they would have been concealed from ordinary observation. In Mr. Darwin's

paper you will find many other facts elucidating the rise of land at Valparaiso, and he has also treated of the general question of the elevation of the whole coast of the Pacific from Peru to Terra del Fuego. Beds of shells were traced by him at various heights above the sea, some a few yards, others 500 or even 1300 feet high, the shells being in a more advanced state of decomposition in proportion to their elevation. Mr. Darwin also shows that parallel terraces such as those of Coquimbo, described by Captain Basil Hall and others, which rise to the height of 300 feet and more, are of marine origin, being sometimes covered with sea-shells, and they indicate successive elevations. There are also grounds for believing that the modern upheaval of land has proceeded not only by sudden starts during convulsions of the earth, but also by insensible degrees in the intervals between earthquakes, as is now admitted to be the case in parts of Norway and Sweden.

This gradual and insensible rising is supposed to affect, not only the region of the Andes, but also the opposite or eastern coast of South America, where earthquakes are never experienced: for the Pampas of Buenos Ayres bear marks of having risen to their present height during a comparatively modern period, while the coast line of the Pacific, or the region of earthquakes and volcanic eruptions, has been the theatre of more violent movements.

It is curious to reflect that if in one portion of a large area of the earth's surface a rise of land takes place at the rate of a few inches in a century, as around Stockholm, while in another portion of the same area land is uplifted about a yard during an equal period, there will be caused, if sufficient time be allowed, a group or chain of lofty mountains in one place, and in the other a low country like the Pampas of South America.

Evidence of a sinking down of land, whether sudden or gradual, is usually more difficult to obtain than the signs of upheaval. I shall therefore mention some facts which have been lately communicated to me by Professor Nilsson, from which it appears that Scania, or the southernmost part of Sweden, has been slowly subsiding for several centuries, in the same manner as was lately shown to be the case with part of Greenland. In the first place there are no elevated beds of recent marine shells in Scania, like those near Stockholm and further to the north. Linnæus, with a view of ascertaining whether the waters of the Baltic were retiring from the Scanian shore, measured in 1749 the distance between the sea and a large stone near Trelleborg. Now Mr. Nilsson informs me that this same stone is a hundred feet nearer the water's edge than it was in Linnæus's time, or eighty-seven years before. He also states that there is a submerged peat moss, consisting of land and freshwater plants, beneath the sea at a point to which no peat could have been drifted down by any river. But, what is still more conclusive, it is found that in sea-port towns, all along the coast of Scania, there are streets below the high-water level of the Baltic, and in some cases below the level of the lowest tide. Thus when the wind is high at Malmö the water overflows one of the present streets, and some years ago

some excavations showed an ancient street in the same place eight feet below, and it was then seen that there had evidently been an artificial raising of the ground, doubtless in consequence of that subsidence. There is also a street at Trelleborg and another at Skanör a few inches below high-water mark; and a street at Ystad is just on a level with the sea, at which it could not have been originally built. I trust that we shall soon receive more circumstantial details of these curious phænomena, which are the more interesting because it has been shown that the elevatory movement in Sweden diminishes in intensity as we proceed southward from the North Cape to Stockholm, from which it seems probable that after passing the line or axis of least movement, where the land is nearly stationary, a movement may be continued in an opposite direction, and thus cause the gradual sinking of Scania.

I cannot take leave of this subject without remarking that the occurrence in various parts of Ireland, Scotland, and England, of recent shells in stratified gravel, sand, and loam, confirm the opinion which I derived from an examination of part of Sweden, namely, that the formations usually called diluvial have not been produced by any violent flood or débacle, or transient passage of the sea over the land, but by a prolonged submersion of the land, the level of which has been greatly altered at periods very modern in our geological chronology. I now believe that by far the greatest part of the dispersion of transported matter has been due to the ordinary moving power of water, often assisted by ice, and cooperating with the alternate upheaval and depression of land. I do not mean wholly to deny that some sudden rushes of water and partial inundations of the sea have occurred, but we are enabled to dispense with their agency more and more in proportion as our knowledge increases.

ORGANIC REMAINS.

Gentlemen, you have been already informed that the Council have this year awarded two Wollaston Medals, one to Captain Proby Cautley of the Bengal Artillery, and the other to Dr. Hugh Falconer, Superintendent of the Botanic Garden at Saharunpore, for their researches in the geology of India, and more particularly their discovery of many fossil remains of extinct quadrupeds at the southern foot of the Himalaya mountains. At our last Anniversary I took occasion to acknowledge a magnificent present, consisting of duplicates of these fossils, which the Society had received from Captain Cautley, and since that time other donations of great value have been transmitted by him to our museum. These Indian fossil bones belong to extinct species of herbivorous and carnivorous mammalia, and to reptiles of the genera crocodile, gavial, emys, and trionyx, and to several species of fish, with which shells of freshwater genera are associated, the whole being entombed in a formation of sandstone, conglomerate, marl, and clay, in inclined stratification, composing a range of hills called the Siwâlik, between the rivers Sutledge and Ganges. These hills rise to the height of

from 500 to 1000 feet above the adjacent plains, some of the loftiest peaks being 3000 feet above the level of the sea.

When Captain Cautley and Dr. Falconer first discovered these remarkable remains their curiosity was awakened, and they felt convinced of their great scientific value; but they were not versed in fossil osteology, and being stationed on the remote confines of our Indian possessions, they were far distant from any living authorities or books on comparative anatomy to which they could refer. The manner in which they overcame these disadvantages, and the enthusiasm with which they continued for years to prosecute their researches when thus isolated from the scientific world is truly admirable. Dr. Royle has permitted me to read a part of their correspondence with him when they were exploring the Siwalik mountains, and I can bear witness to their extraordinary energy and perseverance. From time to time they earnestly requested that Cuvier's works on osteology might be sent out to them, and expressed their disappointment when, from various accidents, these volumes failed to arrive. The delay perhaps was fortunate, for being thrown entirely upon their own resources, they soon found a museum of comparative anatomy in the surrounding plains, hills, and jungles, where they slew the wild tigers, buffalos, antelopes, and other Indian quadrupeds, of which they preserved the skeletons, besides obtaining specimens of all the genera of reptiles which inhabited that region. They were compelled to see and think for themselves while comparing and discriminating the different recent and fossil bones, and reasoning on the laws of comparative osteology, till at length they were fully prepared to appreciate the lessons which they were taught by the works of Cuvier. In the course of their labours they have ascertained the existence of the elephant, mastodon, rhinoceros, hippopotamus, ox, buffalo, elk, antelope, deer, and other herbivorous genera, besides several canine and feline carnivora. On some of these Dr. Falconer and Captain Cautley have each written separate and independent memoirs. Captain Cautley, for example, is the author of an article in the Journal of the Asiatic Society, in which he shows that two of the species of mastodon described by Mr. Clift are, in fact, one, the supposed difference in character having been drawn from the teeth of the young and adult of the same species. I ought to remind you that this same gentleman was the discoverer, in 1833, of the Indian Herculaneum or buried town near Behat, north of Seharunpore, which he found seventeen feet below the surface of the country when directing the excavation of the Doab Canal*.

But I ought more particularly to invite your attention to the joint paper by Dr. Falconer and Captain Cautley on the Sivatherium, a new and extraordinary species of mammalia, which they have minutely described and figured, offering at the same time many profound speculations on its probable anatomical relations. The characters of this genus are drawn from a head almost complete, found

* Journ. of Asiatic Society, Nos. xxv. and xxix. 1834. Principles of Geology, 4th and subsequent editions. See Index, Behat.

at first enveloped in a mass of hard stone, which had lain as a boulder in a water-course, but after much labour the covering of stone was successfully removed, and the huge head now stands out with its two horns in relief, the nasal bones being projected in a free arch, and the molars on both sides of the jaw being singularly perfect. This individual must have approached the elephant in size. The genus Sivatherium, say the authors, is the more interesting, as helping to fill up the important blank which has always intervened between the ruminant and pachydermatous quadrupeds; for it combines the teeth and horns of a ruminant, with the lip, face, and probably proboscis of a pachyderm. They also observe, that the extinct mammiferous genera of Cuvier were all confined to the Pachydermata, and no remarkable deviation from existing types had been noticed by him among fossil ruminants, whereas the sivatherium holds a perfectly isolated position, like the giraffe and the camels, being widely remote from any other type.

I have not space to enter upon the warm discussion which has arisen in France between MM. Blainville and Geoffroy St. Hilaire respecting the amount of analogy which exists between the Sivatherium and the Giraffe; but I observe with pleasure that in the course of that controversy those distinguished naturalists do justice to the zeal and talents displayed by our countrymen Captain Cautley and Dr. Falconer, and to the services which they have rendered to science.*

While these discoveries were made on the banks of the tributaries of the Indus and the Ganges, Mr. Darwin was employed in collecting the bones of large extinct mammalia, near the banks of the Rio Plata, in the Pampas of Buenos Ayres and in Patagonia. Mr. Owen has enabled me to announce to you in a few words some of the most striking results which he has obtained from his examination of the specimens liberally presented by Mr. Darwin to the College of Surgeons, and of which casts will soon be made for our own and other public museums. In the first place, besides a cranium with teeth of the Megatherium, Mr. Darwin has brought home portions of another animal as large as an ox, and allied to the Megatherium. Fragments of its armour are preserved, as well as its jaws, femur, and other bones. There is also a third creature of the order Edentata, and belonging to this same family of Dasypodidæ, in the shape of a gigantic Armadillo, as large as a Tapir. Of the ruminant order there is also a no less remarkable representative in the remains of a gigantic Llama from the plains of Patagonia, which must have been as large as a camel and with a longer neck: and lastly, of the Rodentia there is the cranium of a huge animal of the size of a rhinoceros, with some modification in the form of the skull resembling that in the Wombat.

These fossils, of which a description will shortly be given to the Society by Messrs. Clift and Owen, establish the fact that the peculiar type of organization which is now characteristic of the South American mammalia has been developed on that continent for a

* L. and E. Phil. Mag. vol. ix. p. 193, *et seq.*

long period, sufficient at least to allow of the extinction of many large species of quadrupeds. The family of the armadillos is now exclusively confined to South America and here we have from the same country the Megatherium, and two other gigantic representatives of the same family. So in the Camelidæ, South America is the sole province where the genus Auchenia or Llama occurs in a living state, and now a much larger extinct species of Llama is discovered. Lastly, among the rodents, the largest in stature now living is the Capybara, which frequents the rivers and swamps of South America and is of the size of a hog. Mr. Darwin now brings home from the same continent the bones of a fossil rodent not inferior in dimensions to the rhinoceros.

These facts elucidate a general law previously deduced from the relations ascertained to exist between the recent and extinct quadrupeds of Australia; for you are aware that to the westward of Sydney on the Macquarie River, the bones of a large fossil kangaroo and other lost marsupial species have been met with in the ossiferous breccias of caves and fissures.

A cavern has lately been examined at Yealm Bridge, six miles south-east from Plymouth, by one of our members, Lieut. Col. Mudge, R.E., from whose account it appears that the bones of hyænas are very numerous there. They are associated with those of the elephant, rhinoceros, horse, and other animals usually found in caves. The number of fossil Carnivora, such as the hyæna, wolf, fox, and bear, which have now been met with in districts of cavernous limestone in Great Britain, is so great that we are the more struck with the rarity and general absence of such remains in surrounding and intervening districts, over which the same beasts of prey must have ranged. The Pachydermata, as the elephant, rhinoceros, and hippopotamus, are often discovered in ancient alluvial or fluviatile deposits; but had there been no caves and fissures we should scarcely have obtained any information respecting the existence of lions, tigers, hyænas, and other beasts of prey which inhabited the country at the same period.

The remains of at least two distinct Saurian animals have been discovered by Dr. Riley and Mr. Samuel Stutchbury, in the dolomitic conglomerate of Durdham Down near Bristol. They are allied to the Iguana and Monitor, but the teeth, vertebræ, and other bones exhibit characters by which they are seen to be generically distinct from all existing reptiles. They are particularly deserving of your attention as occurring in the bottom of the magnesian limestone formation, the oldest strata in which the bones of reptiles have as yet been found in Great Britain. The most ancient examples of fossil reptiles known on the continent of Europe occur also in the zechstein of Germany, a formation of about the same age.

I alluded last year to a memoir of Sir Philip Egerton's, in which he pointed out some peculiarities in the structure of the cervical vertebræ of the Ichthyosaurus. He has now proved that in all the species of this genus there are three accessory bones, which he proposes to call, from their shape and position, subvertebral wedge

bones. They are supplementary to the atlas, axis, and third vertebra of the neck, and seem to have escaped the observation of Cuvier and other osteologists.

Mr. Lewis Hunton has communicated to the Society an elaborate account of a section of the upper lias and marlstone in Yorkshire, showing that different beds in those formations are characterized by particular species of Ammonites and other Testacea, each species having a limited vertical range. His observations are valuable not only as illustrating the distribution of fossils on the coast near Whitby, but also as furnishing a point of comparison between that district and many others in Great Britain. Mr. W. C. Williamson of Manchester has had the same object in view in studying the fossils of the oolitic formations of the coast of Yorkshire, and informs us, as the result of his patient investigation, that although certain assemblages of fossils abound in particular subdivisions of the oolite, many species range from the lowermost to nearly the highest beds. This inference is confirmed when we compare the lists drawn up by Mr. Williamson, and those published by Professor Phillips and other competent authorities. Thus some of the shells of the inferior oolite, mentioned in Mr. Williamson's list (*Trigonia gibbosa*, for example), occur also in the Portland-stone of Wiltshire; another, as *Ostrea Marshii*, is characteristic of the cornbrash in the same county; others pass downwards to the lias, as *Orbicula reflexa* and *Ammonites striatulus*. If you consult the tables of organic remains which Dr. Fitton has annexed to his excellent monograph on the strata below the chalk, just published in our Transactions, (2nd Series, vol. iv. part 2.) you will see that a considerable number of shells pass from the upper oolitic groups into the green-sand. We are not to conclude from these facts that certain sets of fossils may not serve as good chronological tests of geological periods, but we must be cautious not to attach too much importance to particular species, some of which may have a wider, others a more limited vertical range. The phænomena alluded to are strictly analogous to those with which we are familiar in the more modern deposits, where different tertiary formations contain some peculiar Testacea, together with others common to older or newer groups, or where shells of species now living in the sea are associated with others that are extinct.

An assemblage of fossil shells has been presented to our museum by Mr. J. Leigh and Mr. J. W. Binney, found at Collyhurst near Manchester, in red and variegated marls, which were referred by them at first to the upper division of the new red sandstone group; but Professors Sedgwick and Phillips consider them to be a red and variegated deposit, belonging to the magnesian limestone series. As these fossils are new and characteristic of a particular subdivision of the beds between the lias and coal, it is to be hoped that they will soon be described and figured.

The petrifaction of wood, and more especially its silicification, still continues to present obscure problems to the botanist and

chemist. The first step towards their solution will probably be made by carefully examining vegetables in different stages of petrifaction; and with this view Mr. Stokes has procured several specimens of wood, partly mineralized and partly not. Among these is a piece found in an ancient Roman aqueduct in Westphalia, in which some portions are converted into spindle-shaped bodies consisting of carbonate of lime: while the rest of the wood remains in a comparatively unchanged state. The same author has pointed out cases both of siliceous and calcareous fossils where the lapidifying process must have commenced at a number of separate points, so as to produce spherical or fusiform petrifactions, independent of each other, in which the woody structure is apparent, while in the intervening spaces the wood has decayed, having after removal been replaced by mineral matter. In some petrifactions the most perishable, in others the most durable portions of plants are preserved, variations which doubtless depend on the time when the mineral matter was supplied. If introduced immediately on the first commencement of decomposition, then the most destructible parts are lapidified, while the more durable do not waste away till afterwards, when the supply has failed, and so never become petrified. The converse of these circumstances gives rise to exactly opposite results. As to the manner in which the minutest pores and fibres discoverable by the microscope, even the spiral vessels themselves can be turned into stone, or have their forms faithfully represented by inorganic matter, no satisfactory explanation has ever yet been offered. In considering, however, this question, you will do well to consult the important suggestion which a celebrated chemist, our late lamented Secretary, Dr. Turner, has thrown out on the application of chemistry to geology. He reminds us that whenever the decomposition of an organic body has begun, the elements into which it is resolved are set free in a state peculiarly adapting them to enter into new chemical combinations. They are in what is technically termed a nascent state, the constituent molecules being probably of extreme smallness and in a fluid or gaseous form, ready to obey the slightest impulse of chemical affinity, so that if the water percolating a stratum be charged with mineral ingredients, and come in contact with elements thus newly set free, a mutual action takes place, and new combinations result, in the course of which solid particles are precipitated so as to occupy the place left vacant by the decomposed organic matter. In a word, all the phænomena attendant on slow putrefaction must be studied whenever we attempt to reason on the conversion of fossil bodies into stone; and in regard to silicification, Dr. Turner has shown how great a quantity of silex is set free as often as felspar decomposes, and how abundantly siliceous matter may be imparted from this source alone to running water throughout the globe.

As I have mentioned the name of Dr. Turner, I cannot pass on without an expression of sorrow for the untimely death of that amiable and distinguished philosopher. Mr. Whewell and Mr. Murchison alluded in most feeling terms this morning at the Ge-

neral Meeting to this melancholy event, which is too recent and too painful to myself and others to allow me now to dwell longer upon it.

Before quitting the subject of vegetable petrifactions, I ought to mention a memoir just published, by Mr. H. R. Göppert, Professor of Botany at Breslau, "On the various Conditions in which Fossil Plants are found, and on the Process of Lapidification*." He has instituted a series of most curious experiments, and his success in producing imitations of fossil petrifactions has been very remarkable. I have only space to allude to one or two examples. He placed recent ferns between soft layers of clay, dried these in the shade, and then slowly and gradually heated them, till they were red hot. The result was the production of so perfect a counterpart of fossil plants as might have deceived an experienced geologist. According to the different degrees of heat applied, the plants were obtained in a brown or perfectly carbonized condition, and sometimes, but more rarely, they were in a black shining state, adhering closely to the layer of clay. If the red heat was sustained until all the organic matter was burnt up, only an impression of the plant remained.

The same chemist steeped plants in a moderately strong solution of sulphate of iron, and left them immersed in it for several days until they were thoroughly soaked in the liquid. They were then dried and kept heated until they would no longer shrink in volume, and until every trace of organic matter had disappeared. On cooling them he found that the oxyd formed by this process had taken the form of the plants. Professor Göppert then took fine vertical slices of the Scotch fir, *Pinus sylvestris*, and treated them in the same way; and so well were they preserved, that, after heating, the dotted vessels so peculiar to this family of plants were distinctly visible. A variety of other experiments were made by steeping animal and vegetable substances in siliceous, calcareous, and metallic solutions, and all tended to prove that the mineralization of organic bodies can be carried much further in a short time than had been previously supposed.

These experiments seem to open a new field of inquiry, and will, I trust, soon be repeated in this country. In endeavouring, however, to verify them, the greatest caution will be required, or we may easily be deceived. We must ascertain, for example, with certainty that every particle of animal or vegetable matter is driven off before we attempt to determine the full extent to which mineralization may have proceeded. Professor Göppert is doubtless aware that coniferous wood may be burnt and reduced to charcoal, and after havnig been kept for some time at a red heat, will continue to exhibit, on being cooled, the discs or reticulated structure to which he alludes. If, therefore, some small particles of carbon remain in the midst of the oxide of iron, such portions may retain traces of the vessels peculiar to coniferous wood; and an observer not on his guard,

* Poggendorff, Annalen der Physik und Chemie, vol. xxxviii. part 4. Leipsic, 1836.

might infer that the same structure was preserved throughout the mass.

In my last address, I alluded to Mr. Lonsdale's detection of vast numbers of microscopic corallines and minute shells in the substance of the white chalk of various counties in England, where this rock had not been suspected of consisting of recognisable organic bodies. I cannot deny myself the pleasure of mentioning the still more singular and unexpected facts brought to light during the last year, by Professor Ehrenberg of Berlin, respecting the origin of tripoli. I need scarcely remind you, that tripoli is a rock of homogeneous appearance, very fragile and usually fissile, almost entirely formed of flint, and which was called polir-schiefer, or polishing slate, by Werner, being used in the arts for polishing stones or metals. There have been many speculations in regard to its origin, but it was a favourite theory of some geologists that it was a siliceous shale hardened by heat. The celebrated tripoli of Bilin in Bohemia consists of siliceous grains united together without any visible cement, and is so abundant that one stratum is no less than fourteen feet thick. After a minute examination of this as well as of the tripoli from Planitz in Saxony, and another variety from Santa Fiora in Tuscany, and one from the Isle of France, Ehrenberg found that the stone is wholly made up of millions of siliceous cases and skeletons of microscopic animalcules. It is probably known to you, that this distinguished physiologist has devoted many years to the anatomical investigation of the infusoria, and has discovered that their internal structure is often very complicated, that they have a distinct muscular and nervous system, intestines, sexual organs of reproduction, and that some of them are provided with siliceous shells, or cases of pure silex. The forms of these durable shells are very marked and various, but constant in particular genera and species. They are almost inconceivably minute, yet they can be clearly discerned by the aid of a powerful microscope, and the fossil species preserved in tripoli are seen to exhibit in the family Bacillaria and some others the same divisions and transverse lines which characterize the shells of living infusoria.

In the Bohemian schist of Bilin, and in that of Planitz in Saxony, both of them tertiary deposits, the species are freshwater, and are all extinct. The tripoli of Cassel appears to be more modern, and the infusoria in that place, which are also freshwater, are some of them distinctly identical with living species, and others not. In the tripoli brought from the Isle of France, the cases or shells all belong to well-known recent marine species.

The flinty shells of which we are speaking although hard are very fragile, breaking like glass, are therefore admirably adapted when rubbed for wearing down into a fine powder fit for polishing the surface of metals. It is difficult to convey an idea of their extreme minuteness, but I may state that Ehrenberg estimates that in the Bilin tripoli there are 41,000 millions of individuals of the *Gaillonella distans* in every cubic inch of stone. At

every stroke therefore of the polishing stone we crush to pieces several thousands if not myriads of perfect fossils.

Gentlemen,—Although I have already extended this Address beyond the usual limits, I cannot conclude without congratulating you on the appearance of Dr. Buckland's Bridgewater Treatise, a work in the execution of which the author has most skilfully combined several distinct objects. He has briefly explained the manner in which the materials of the earth's crust are arranged, and the evidence which that arrangement affords of contrivance, wisdom, and foresight. He has also given us a general view of the principal facts brought to light by the study of organic remains; thus contributing towards the filling up one of the greatest blanks which existed in the literature of our science, while at the same time he has pointed out the bearing of these phænomena on natural theology.

He has shown that geology affords one kind of testimony perfectly distinct from natural history of the adaptation of particular means and forces to the accomplishment of certain ends for which the habitable globe has been framed. These proofs are illustrated in the author's chapters on the origin and mechanism of springs, on the distribution of metallic and other minerals in the earth, and the position of coal in stratified rocks. In reference to these points it is demonstrated that some even of the most irregular forces have produced highly beneficial results, in modifying the subterranean economy of the globe. But I shall not dwell on this part of the Treatise, but pass on at once to that which constitutes the body of the work, and which relates to palæontology.

In considering this department, the number and variety of objects which offer themselves to the naturalist are so great, that the choice was truly embarrassing. Dr. Buckland has judiciously selected a few of the most striking examples from each of the great classes of organic remains, and when speaking of extinct animals, has explained the method by which the anatomist and physiologist have been able to restore the organization of the entire individual, by reasoning from the evidence afforded by a few bones or other relics preserved in a fossil state. He has described the parts of the living animal or plant most nearly analogous to those which are found buried in the earth, usually illustrating by figures the distinctness and at the same time the resemblance of the recent and extinct species, showing that all are parts of one great scheme, and that the lost species even supply links which are wanting in the existing chain of animal and vegetable creation.

It is impossible to read the account given of the Megatherium, and to contrast it with that drawn up by Cuvier of the same species, without being struck with the increased interest and instruction, and the vast accession of power derived from viewing the whole mechanism of the skeleton in constant relation to the final causes for which the different organs were contrived.

The chapter on saurian and other reptiles has afforded the Professor another beautiful field for exemplifying the infinite variety of

mechanical contrivances and combinations of form and structure which the fossil representatives of that class exhibit.

The account also of the Cephalopodous Mollusca, so many thousands of which are scattered through the strata, and which until very recently have presented so obscure a problem to the naturalist, is full of original observation. The history of the animals which formed the Belemnites, of which it appears that nearly one hundred species are now known, and the proofs adduced that they were provided with ink-bags like the cuttle-fish, the description also of the fossil pen-and-ink fish, or Loligo, and other sections of this part of the Treatise, carry our information respecting the family of naked Cephalopods much further than was ever attempted in any previous work. Nor should I omit to mention the exposition of an ingenious theory for the use of the siphuncle and air-chambers of the Ammonite, which, whether confirmed by future examination or not, becomes in the author's hands the means of conveying to the reader a clear and well-defined notion of the varied forms and complicated structure of these shells, and of awakening a lively desire to understand their singular organization.

I may also recall to your notice the just and striking manner in which certain physical inferences are drawn from the conformation of the eyes of extinct Crustacea, such as the Trilobite. The most delicate parts of these organs are sometimes found petrified in rocks of high antiquity, and it is justly observed, that such optical instruments give information regarding the condition of the ancient sea and ancient atmosphere, and the relations of both these media to light. The fluid in which these marine animals lived at remote periods must have been pure and transparent to allow the passage of light to organs of vision resembling those of living Crustaceans; and this train of reasoning naturally leads us still further, and to more important consequences, when we reflect on the general adoption of the undulatory theory of light, and the connexion between light, heat, electricity, and magnetism.

I have heard it objected, that the zoologist and botanist had already advanced such abundant proofs of design in the construction of living animals, and plants, that the auxiliary evidence of palæontology was useless, and that to appeal to fossils in support of the same views was to add weaker to stronger arguments. In the living animal, it is said, we can study its entire organization, observe its habits, see the manner in which it applies each organ, and so verify with certainty the ends for which any particular member was formed and fashioned. But in the case of the fossil, we have first to infer the greater part of the organization from such parts as alone remain, and then further to infer from analogy the habits and functions discharged, and lastly the former conditions of existence of the creatures so restored. If then we occasionally fall into error when speculating on the use of the organs of living species, how much more easily may we be deceived in regard to the fossil!

In answering this objection, it cannot be denied that the data

supplied by palæontology are less complete; but they are nevertheless abundantly sufficient to establish a very close analogy between extinct and recent species, so as to leave no doubt on the mind that the same harmony of parts and beauty of contrivance which we admire in the living creature has equally characterized the organic world at remote periods. If this be granted, it is enough; the geologist can then bring new and original arguments from fossil remains to bear on that part of natural theology which seeks to extend and exalt our conceptions of the intelligence, power, wisdom, and unity of design manifested in the creation.

It can now be shown that the configuration of the earth's surface has been remodelled again and again; mountain chains have been raised or sunk, valleys have been formed, again filled up, and then re-excavated, sea and land have changed places, yet throughout all these revolutions, and the consequent alterations of local and general climate, animal and vegetable life has been sustained. This appears to have been accomplished without violation of those laws now governing the organic creation, by which limits are assigned to the variability of species. There are no grounds for assuming that species had greater powers of accommodating themselves to new circumstances in ancient periods than now. The succession of living beings was continued by the introduction into the earth from time to time of new plants and animals. That each assemblage of new species was admirably adapted for successive states of the globe, may be confidently inferred from the fact of the myriads of fossil remains preserved in strata of all ages. Had it been otherwise, had they been less fitted for each new condition of things as it arose, they would not have increased and multiplied and endured for indefinite periods of time.

Astronomy had been unable to establish the plurality of habitable worlds throughout space, however favourite a subject of conjecture and speculation; but geology, although it cannot prove that other planets are peopled with appropriate races of living beings, has demonstrated the truth of conclusions scarcely less wonderful, the existence on our own planet of many habitable surfaces, or worlds as they have been called, each distinct in time, and peopled with its peculiar races of aquatic and terrestrial beings.

Thus as we increase our knowledge of the inexhaustible variety displayed in living nature, and admire the infinite wisdom and power which it displays, our admiration is multiplied by the reflection that it is only the last of a great series of pre-existing creations of which we cannot estimate the number or limit in past time.

All geologists will agree with Dr. Buckland, that the most perfect unity of plan can be traced in the fossil world throughout all the modifications which it has undergone, and that we can carry back our researches distinctly to times antecedent to the existence of man. We can prove that man had a beginning, and that all the species now contemporary with man, and many others which preceded, had also a beginning; consequently the present state of the

organic world has not gone on from all eternity as some philosophers had maintained.

But when conceding the truth of these propositions, I am prepared to contest another doctrine which the Professor advocates, namely, that by the aid of geological monuments we can trace back the history of our terraqueous system to times anterior to the first creation of organic beings. If it was reasonable that Hutton should in his time call in question the validity of such a doctrine, whether founded on the absence of organic remains in strata called primary or in granite, still more are we bound, after the numerous facts brought to light by modern geology, to regard the opinion as more than questionable. I observe with pleasure that Dr. Buckland broadly assumes what I have elsewhere termed the metamorphic theory, having stated in his 6th chapter that beds of mud, sand, and gravel, deposited at the bottom of ancient seas, have been converted by heat and other subterranean causes into gneiss, mica slate, hornblende slate, clay slate, and other crystalline schists. But if this transmutation be assumed, it must also be admitted that the obliteration of the organic remains, if present, would naturally have accompanied so entire a change in mineral structure. The absence, then, of organic fossils in crystalline stratified rocks, of whatever age, affords no presumption in favour of the non-existence of animals and plants at remote periods.

The author, however, in another part of his Treatise contends, that even if the strata called primary once contained organic remains, there is still evidence in the fundamental granite of an antecedent universal state of fusion, and consequently a period when the existence of the organic world, such as it is known to us, was impossible. There was, he says, one universal mass of incandescent elements, forming the entire substance of the primæval globe, wholly incompatible with any condition of life which can be shown to have ever existed on the earth*. Believing as I do in the igneous origin of granite, I would still ask, what proof have we in the earth's crust of a state of total and simultaneous liquefaction either of the granitic or other rocks, commonly called plutonic? All our evidence, on the contrary, tends to show that the formation of granite, like the deposition of the stratified rocks, has been successive, and that different portions of granite have been in a melted state at distinct and often distant periods. One mass was solid, and had been fractured before another body of granitic matter was injected into it, or through it in the form of veins. In short, the universal fluidity of the crystalline foundations of the earth's crust can only be understood in the same sense as the universality of the ancient ocean. All the land has been under water, but not all at one time; so all the subterranean unstratified rocks to which man can obtain access have been melted, but not simultaneously.

Nor can we affirm that the oldest of the unstratified rocks

* Buckland's Bridgewater Treatise, vol. i. p. 55.

hitherto discovered is more ancient than the oldest stratified formations known to us; we cannot even decide the relations in point of age of the most ancient granite to the oldest *fossiliferous* beds.

But why, I may ask, should man, to whom the early history of his own species and the rise of nations presents so obscure a problem, feel disappointed if he fail to trace back the animate world to its first origin? Already has the beginning of things receded before our researches to times immeasurably distant. Why then, after wandering back in imagination through a boundless lapse of years, should we expect to find any resting-place for our thoughts, or hope to assign a limit to the periods of past time throughout which it has pleased an omnipotent and eternal Being to manifest his creative power?

But it is not my intention to advert now to these and other points on which I happen to differ from Dr. Buckland. I would rather express the gratification I feel in finding myself in perfect accordance with him on so many subjects. His work is admirably adapted to convey instruction on organic remains, and other departments of geology, both to beginners and to those well versed in the science, and is characterized throughout by a truly philosophical spirit, which betrays no desire to adhere tenaciously to dogmas impugned or refuted by the modern progress of science. On the contrary, the author has abandoned several opinions which he himself had formerly advocated; and although still attached to the theory which teaches the turbulent condition of the planet when the lias and other fossiliferous rocks were formed, and the general insufficiency of existing causes to explain the changes which have occurred on the earth, he yet refers in almost all parts of his book to the ordinary operations of nature to explain a variety of phænomena once supposed to be the result of causes different in kind and degree from those now acting.

I have now, Gentlemen, only to offer you my acknowledgements for the high honour conferred upon me by my election to fill the President's chair for the last two years; and it is a source of great satisfaction to me to feel assured of the continued prosperity and usefulness of the association when I resign my trust into the hands of a successor so distinguished for his zeal, talents, and varied acquirements as Mr. Whewell.

LXXVIII. *On the Thermo-electric Spark, &c. Communicated by* C. Wheatstone, *Esq., F.R.S., Professor of Experimental Philosophy in King's College, London.*

THE following notice of some recent experiments made in Italy, on the production of the thermo-electric spark, and on the chemical effects of the thermo-electric currents, will, no doubt be acceptable to many of your readers. I shall

confine myself to a simple statement and corroboration of the facts, avoiding all theoretical considerations.

The Cav. Antinori, Director of the Museum at Florence, having heard that Prof. Linari, of the University of Siena, had succeeded in obtaining the electric spark from the torpedo by means of an electro-dynamic helix and a temporary magnet, conceived that a spark might be obtained by applying the same means to the thermo-electric pile. Appealing to experiment his anticipations were fully realized. No account of the original investigations of Antinori has yet reached this country, but Prof. Linari, to whom he early communicated the results he had obtained, immediately repeated them, and published the following additional observations of his own in *L'Indicatore Sanese*, No. 50, of Dec. 13, 1836.

"1. With an apparatus consisting of temporary magnets and electro-dynamic spirals, the wire of which was 505 feet in length, he obtained a brilliant spark from a thermo-electric pile of Nobili's construction consisting only of 25 elements, which was also observed in open daylight.

"2. With a wire 8 feet long coiled into a simple helix, the spark constantly appeared in the dark, on breaking contact, at every interruption of the current; with a wire 15 inches long he saw it seldom, but distinctly; and with a double pile, even when the wire was only 8 inches long. In all the above-mentioned cases the spark was observed only on breaking contact, however much the length of the wire was diminished.

"3. The pile consisting merely of these few elements, and within such restricted limits of temperature as those of ice and boiling water, readily decomposed water. Short wires were employed having oxidizable extremities; the hydrogen was sensibly evolved at one of the poles.

"4. A mixture of marine salt moistened with water and of nitrate of silver being placed between two small horizontal plates of gold, communicating respectively with the wires of the pile, the latter after having acted on the mixture gave evident signs of the appearance of revivified silver on the plate which was next the antimony.

"5. An unmagnetic needle placed within a close helix formed by the wire of the circuit became well magnetized by the current.

"6. Under the action of the same current the phænomenon of the palpitation of mercury was distinctly observed."

The interesting nature of these experiments induced me to attempt to verify the principal result. The thermo-electric pile I employed consisted of 33 elements of bismuth and an-

timony formed into a cylindrical bundle $\frac{3}{4}$ of an inch in diameter and $1\frac{1}{3}$ inch in length; the poles of this pile were connected by means of two thick wires with a spiral of copper ribbon 50 feet in length and $1\frac{1}{2}$ inch broad, the coils being well insulated by brown paper and silk. One face of the pile was heated by means of a red-hot iron brought within a short distance of it, and the other face was kept cool by contact with ice. Two stout wires formed the communication between the poles of the pile and the spiral, and the contact was broken, when required, in a mercury cup between one extremity of the spiral and one of these wires. Whenever contact was thus broken a small but distinct spark was seen, which was visible even in daylight. Professors Daniell, Henry, and Bache assisted in the experiment, and were all equally satisfied of the reality of the appearance.

At another trial the spark was obtained from the same spiral connected with a small pile of 50 elements, on which occasion Dr. Faraday and Prof. Johnston were present, and verified the fact. On connecting two such piles together so that the similar poles of each were connected with the same wires, the same was seen still brighter*.

I conclude, therefore, that the experiment of Antinori is a real addition to our knowledge of electrical phænomena, and though it was far from being unexpected, it supplies a link that was wanting in the chain of the experimental evidence which tends to prove that electricity, from sources however varied, is similar in its nature and effects, a conclusion rendered more than probable by the recent discoveries of Faraday. The effects thus obtained from the electric current originating in the thermo-electric pile may no doubt be easily exalted by those who have the requisite apparatus at their disposal. It is not too much to expect, seeing the effects produced by a pile of such small dimensions, that by proper combinations the effects may be exalted to equal those of an ordinary voltaic pile.

I shall close this hasty communication with a notice of some experiments on the chemical action of the thermo-electric pile made earlier by Prof. G. D. Botto, of the University of Turin. The form of the pile he employed may suggest some useful hints to those who are inclined to continue the inquiry, as it admits of the application of much higher degrees of heat than one of the ordinary construction does, though the difference of the thermo-electric relations of the two metals employed is not

* The two piles here employed were made by Mr. Newman of Regent-street.

so considerable. Prof. Botto's experiments were published in the *Bibliothèque Universelle* for September 1832, and I am not aware that they have yet been published in any English Journal. The thermo-electric apparatus was a metallic wire, or chain, consisting of 120 pieces of platinum wire, each one inch in length and $\frac{1}{100}$th of an inch in diameter, alternating with the same number of pieces of soft iron wire of the same dimensions. This wire was coiled as a helix round a wooden rule 18 inches long, in such a manner that the joints were placed alternately at each side of the rule, being removed from the wood at one side to the distance of four lines. Employing a spirit-lamp of the same length as the helix, and one of Nobili's galvanometers, a very energetic current was shown to exist; acidulated water was decomposed, and the decomposition was much more abundant when copper instead of platinum poles were used: in this case hydrogen only was liberated. The current and decomposition were augmented when the joints were heated more highly. Better effects were obtained with a pile of bismuth and antimony, consisting of 140 elements bound together into a parallelopiped, having for its base a square of two inches, three lines, and an inch in height.

King's College, April 24, 1837.

LXXIX. *Intelligence and Miscellaneous Articles.*

ÆTHEREAL OIL OF WINE. LIEBIG AND PELOUSE.

IT is well known that a mixture of alcohol and water in the same proportions as they exist in wine has scarcely any odour, whilst a few drops of wine remaining in a bottle will be easily recognised by its smell. This characteristic odour, which is possessed by all wines in a greater or less degree, is produced by a peculiar substance, which has all the characters of an essential oil. This substance is not to be confounded with the aroma of wine; for it is not volatile, and appears to be different in various kinds of wine, and in the greater number it does not exist at all.

When large quantities of wine are submitted to distillation, an oily substance is obtained towards the end of the operation; it is also procured from wine lees, and especially from that which is deposited in the casks after fermentation has commenced.

This æthereal oil forms about one 40,000dth part of wine. In its original state it has a strong flavour, is usually colourless, but owing to the presence of a small portion of oxide of copper it is sometimes greenish: when this is separated by hydrosulphuric acid it is colourless. The mode of purifying this substance will be mentioned after its composition and principal properties have been described.

This æthereal oil of wine contains a considerable quantity of oxy-

gen; but its constitution is very different from that of the oxygenated essential oils hitherto known. It consists of a new peculiar acid, analogous to the fatty acids, combined with æther; and it of course is one of the class of compound æthers. It is the first instance of the occurrence of an æther which is insoluble in water, and produced during the vinous fermentation without the intervention of the chemist. The strong resemblance which this substance bears to the essential oils, ought to cause them to be studied under the same point of view, and it is probable that light may be thrown thereby upon this class of organic compounds. To the new acid MM. Liebig and Pelouse have given the name of *œnanthic acid*, and to the essential oil *œnanthic æther*.

ŒNANTHIC ÆTHER.

The rough æther contains variable quantities of free acid: as it is more volatile than the acid, it may be obtained free from it by distillation by separating the first fourth of the product. In order to obtain it perfectly pure, it is preferable to shake it frequently with a hot solution of carbonate of soda, which dissolves the free acid without altering the æther. The mixture is milky, and does not become clear by long standing; but if it be boiled for a short time, then the æther floats on the surface of the fluid, and may be easily separated. By agitating it with fragments of chloride of calcium, the small quantity of water or alcohol which it contains is easily separated. The æther thus purified is very fluid, resembling the essential oil of mustard; it is colourless, has an extremely strong smell of wine, and which is almost intoxicating when breathed. Its taste is strong and disagreeable. It dissolves readily in æther and in alcohol, even when the latter is weak; it is not sensibly soluble in water. Its density is 0·862, that of its vapour is 1·0508; it is not very volatile; when distilled with water only, about 100 grains come over with a pound of water. It boils at about 442° Fahr.

It appears to be composed of

Six equivalents of Hydrogen ..	6	
Six equivalents of Carbon	36	
One equivalent of Oxygen....	8	
	—	50

Œnanthic æther is instantaneously decomposed by the caustic alkalis; but the carbonates produce no sensible effect upon it; it is not altered by ammonia, either in solution or in its gaseous state, or even when gently heated.

When it is boiled with caustic potash it disappears in a few seconds, and if the operation be conducted in a retort, a considerable quantity of alcohol is obtained, and there remains a very soluble compound of œnanthic acid and potash. If this compound be decomposed by dilute sulphuric acid, the œnanthic acid immediately separates and forms an oily inodorous stratum on the surface of the liquid.

PREPARATION OF BORON. BY DR. R. D. THOMSON.

"In the process for obtaining boron by the action of potassium on boracic acid, a considerable loss has been generally experienced in consequence of an explosion which usually accompanies the combination. A more œconomical method has therefore been proposed, viz. to decompose an alkaline fluoborate by potassium. It appears to me that there can be only two causes which can produce the explosion in the first mode of preparation—either the presence of water in the boracic acid (as suggested by Dr. Thomson of Glasgow), or the existence of this fluid either in the potassium itself, or in connexion with the same metal. I believe [says Dr. R. D. T.] the latter circumstance to be the cause of the failure of the experiment in most cases. I have succeeded [he continues] in forming pure boron readily by the following plan;—A portion of Tuscan boracic acid was fused in a red heat in a platinum crucible till it became perfectly white; it was then taken out of the crucible and reduced to a granular powder; two parts of potassium were then introduced into a common test-tube. Care should be taken to remove the white crust which usually covers potassium, as it occurs in the country; this coating is hydrate of potash generated by the action of water contained in English naphtha. If German naphtha is employed to preserve the potassium there is little or no hydrate of potash formed. The quantity of water in English naphtha is sometimes so considerable, that I have actually seen potassium take fire when introduced into it. For the specimen of potassium with which the present experiments were made, I am indebted to the kindness of Mr. Graham of Glasgow. To proceed with the process: the potassium, cut into minute fragments, was mixed with one part of the granular boracic acid described; the tube was then cautiously exposed to the flame of a spirit lamp; scanty white fumes began to be discharged; as soon as they ceased to be formed, the mixture was heated to redness, and the process continued for ten minutes; when the tube had cooled, a drop of water was introduced in contact with this mixture by means of a glass rod; no action occurred, showing that no potassium was present; a quantity of water was then introduced into the tube, mixed with some muriatic acid; the tube was washed out, and the contents thrown upon a filter; the boron was well washed and dried; it possessed a fine deep brown colour, and was entirely converted into boracic acid by ignition with nitrate of potash.

The improvements, therefore, in the process for preparing boron now described, consist, 1st, in pointing out the probable cause of former failures, viz. the employment of potassium containing water; and 2nd, the use of boracic acid in a granular or rough state, which enables the decomposition to go on slowly, and thus prevents the rapid union of elements either foreign or essential to the process. We are thus enabled to witness the whole operation; no violent action occurring to prevent the performance of the experiment in a glass tube." *British Annals of Medicine, Feb.* 1837.

EXPERIMENTS ON CAMPHOR.

MM. Dumas and Peligot have made the following statements as the results of their experiments on common camphor: Neutral and oxygenated organic bodies, when their vapour contains half a volume of oxygen, approximate alcohol in general in the nature of their reactions. This is at any rate what happens with the spirit of wood, oil of potatoes, ethal, and pyroacetic spirit. This generalization struck us long since, and we have subjected common camphor, which is so constituted, to the action of some bodies which would allow of procuring from them decisive products, admitting that camphor would act like alcohol. We shall limit ourselves to stating here, that common camphor, treated with anhydrous phosphoric acid, furnishes a liquid volatile oily carburetted hydrogen composed of $C^{40} H^{28}$; this then comes from the camphor, as if this body, being formed of $C^{40} H^{28}, H^4 O^2$, should lose its water under the influence of the phosphoric acid. On acting upon camphor by sulphuric acid, a light volatile oil is also obtained. It appeared to us to be formed of the carburetted hydrogen preceding and camphor, in variable proportions. By rectification with anhydrous phosphoric acid, it resolves always into the carburetted hydrogen $C^{40} H^{28}$ already mentioned. *L'Institut*, April 3, 1837.

COMPOUND OF ALBUMEN AND BICHLORIDE OF MERCURY. BY M. LASSAIGNE.

It has long been known that a solution of bichloride of mercury precipitates a solution of albumen, even when very dilute. This fact, which proves both the strong mutual action of those bodies and the slight solubility of the product, has been employed by Dr. Bostock as a method of distinguishing albumen from gelatin and mucus, and of recognising it in the animal fluids. In 1813, M. Orfila proposed white of egg or albumen as an antidote for corrosive sublimate, conceiving that the insolubility of the product of their reaction, if it would not neutralize, would at least diminish the deleterious property of the mercurial chloride.

M. Orfila considered the precipitate formed as composed of albumen and protochloride of mercury, while M. Chantourelle, in 1822, considered it as a compound of albumen and the bichloride of mercury.

In order to discover the facts of the case, M. Lassaigne undertook some experiments, detailed in a memoir presented to the Institute. He ascertained that the precipitate obtained by mixing an excess of solution of bichloride of mercury with a solution formed one part of white of egg and six parts of water, retained from 81·5 to 82 parts of combined water in 100. He considers the moist precipitate as M. Chantourelle does, as very slightly soluble in water. He ascertained also that it is dissolved by the chloride, bromide, and iodide of potassium, sodium and calcium, and by the

phosphoric, sulphurous, hydrosulphuric, arsenic, acetic, oxalic, tartaric, paratartaric, and malic acids. But the nitric, sulphuric, hydrochloric, hydriodic, and gallic acid are not capable of dissolving it. It is soluble in the cold solutions of potash, soda, ammonia, and lime. These solutions, after some days, yield a deposit of finely divided mercury. M. Lassaigne is of opinion that the alkali gives rise to a chloride or an alkaline hydrochlorate and peroxide of mercury, which dissolves with the albumen in the excess of alkali over that which produces the alkaline chloride or hydrochlorate.

M. Lassaigne thinks that in the precipitation of the albumen by the bichloride of mercury, the two bodies combine integrally, as supposed, but not proved by M. Chantourelle.

The following, among others, are two facts upon which this opinion is founded: 1st. If a proper quantity of protochloride of tin be added to a solution of the albuminous precipitate in water saturated with common salt, a white precipitate of protochloride of mercury is formed; this is precisely the result obtained, as is well known to chemists, from a mixture of protochloride of tin and bichloride of mercury; the excess of chlorine over that which forms a protochloride of mercury, converts the protochloride of tin into bichloride, provided always that that protochloride of tin is not in too great excess, in which case metallic mercury is procured.

2nd. If æther be agitated with the solution of precipitated albumen in water saturated with chloride of sodium, and if the æthereal fluid be separated when it has become clear, there is obtained on evaporating it a residue of bichloride of mercury and chloride of sodium.

In order to justify the conclusion which M. Lassaigne draws from this fact he adds:

a. That æther which is put in contact with protochloride of mercury, not only does not dissolve it, but does not convert it into mercury and bichloride, by the affinity which it might possess for the latter.

b. That the solution of the albuminous mercurial compound in chloride of sodium, having the property of coagulating, like a concentrated aqueous solution of albumen. The corrosive sublimate is found in the water which remains after the coagulation of the original solution; but the author remarks that the greater part of the bichloride remains combined with the coagulated albumen.

c. That even the most finely divided protochloride of mercury does not combine with albumen dissolved in water. M. Lassaigne has made numerous other observations on the mutual action of albumen, of bichloride of mercury, and of chloride of sodium. For example, he has found:

(*a.*) That a solution of two atoms of bichloride of mercury and three of chloride of sodium does not precipitate a solution of albumen;

(*b.*) That albumen which is precipitated cold by bichloride of

mercury, is in the state of albumen which M. Chevreul has called *soluble*, to distinguish from *coagulated albumen* (*l'albumine cuite*), which is insoluble in water;

(*c.*) That a solution of precipitated albumen and bichloride of mercury in a solution of chloride of sodium, is coagulated by heat, like pure albumen, except that the coagulum retains some bichloride, and that this solution evaporated in vacuo, separates from the chloride of sodium in the state of albumen, combined with bichloride of mercury insoluble in water.

Admitting as exact the atomic constitution which Thomson has calculated from the analyses of Thenard and Gay-Lussac, M. Lassaigne considers that the precipitate consists of one atom of bichloride of mercury and ten atoms of albumen, which gives for 100 parts 6·67 of bichloride and 93·33 of albumen.

M. Lassaigne concludes his memoir with an examination of the action of bichloride of mercury upon the fibrin extracted from blood. He shows that a solution of bichloride of mercury in which fibrin has been placed for several days does not contain any free hydrochloric acid, as has been stated; for mercury agitated with the liquor separated from the fibrin, precipitates all the bichloride in the state of protochloride, without leaving any hydrochloric acid in the water. M. Lassaigne has likewise determined the absence of chlorine in the same liquor separated from fibrin. He concludes from this double experiment that fibrin, like albumen, combines with bichloride of mercury without converting it into protochloride. *L'Institut*, April 5, 1837.

ŒNANTHIC ACID.

Œnanthic Acid, separated as just described, is to be carefully washed with hot water. It may be afterwards dried either by shaking it with chloride of calcium, or exposing in vacuo to concentrated sulphuric acid. The hydrated œnanthic acid thus obtained, is perfectly white, and at about 60° is of the consistence of butter, but at a higher temperature it melts, becomes a colourless oil, which is both inodorous and insipid, reddens litmus, dissolves readily in the caustic and carbonated alkalis. This acid, like all the fatty acids, forms two series of salts, one of which is acidulous, without, however, exhibiting any sensible acid reaction, the other is neutral, with a strongly marked alkaline reaction. It is readily soluble in æther and in alcohol. When a hot solution of œnanthic acid is saturated with potash, so that it exhibits neither acid nor alkaline reaction, on cooling there is formed a pasty mass consisting of fine, extremely fine silky needleform crystals, which are the acidulous salt of potash.

When œnanthic acid is dissolved with heat in a solution of carbonate of soda, and the solution is evaporated to dryness, and then treated with alcohol, neutral œnanthate of soda is dissolved, and the

carbonate of soda remains. The solution of the œnanthate forms on cooling a semi-transparent gelatinous mass.

If œnanthic acid be added to a cold solution of acetate of lead, white flocks of an insoluble salt are immediately formed. Acetate of copper produces an analogous decomposition; these are acidulous salts, which are insoluble in water, but dissolve readily in alcohol; they may be obtained in crystals by allowing a saturated alcoholic solution to cool.

It is, however, extremely difficult to obtain by this method the salts free from all adherent acid. If they are washed with alcohol, they are then decomposed into more acidulous salts and subsalts.

Œnanthic acid appeared to be composed of

Thirteen equivalents of Hydrogen	13	
Fourteen equivalents of Carbon	84	
Two equivalents of Oxygen............	16	
	——	113

Its combining weight appears to be about the same, judging from the composition of œnanthate of copper, and supposing it to be composed of one equivalent each of acid and base.

METEOROLOGICAL OBSERVATIONS FOR MARCH 1837.

Chiswick. — March 1—5. Bleak and cold. 6—8. Fine, but cold. 9. Cloudy. 10. Cloudy: rain. 11. Very fine. 12. Clear and frosty: sleet. 13. Cloudy and cold. 14. Very clear: stormy at night. 15. Bleak and cold. 16. Drizzly: hazy. 17—19. Cold and overcast. 20, 21. Snow showers. 22. Cloudy. 23. Cloudy: frosty at night. 24. Very severe frost for the period of the season: clear and cold. 25. Cold and dry. 26—31. Excessively dry and cold.

It will have been observed in the monthly extracts from the Meteorological Journal kept at the garden of the Horticultural Society at Chiswick, that the *extremes* of the *max.* and *min.* columns have hitherto been inserted at the bottom of the columns containing the indications of the barometer and thermometer. Instead of this, in future, at the suggestion of Dr. Lindley, it is proposed to give the *means* of the respective columns, the extremes of highest and lowest being easily found, if required, by inspection. R. Thompson.

Boston.—March 1—3. Cloudy. 4. Stormy. 5. Fine. 6. Cloudy. 7. Fine. 8. Cloudy. 9. Cloudy: rain P.M. 10. Fine. 11. Fine: snow at night. 12. Rain. 13, 14. Fine. 15—18. Cloudy. 19. Fine. 20. Cloudy: snow early A.M. 21. Snow. 22. Fine: snow early A.M. 23. Fine: snow A.M. and P.M. 24, 25. Fine. 26, 27. Fine: snow P.M. 28. Cloudy: snow early A.M. 29. Fine: rain and hail P.M. 30. Fine. 31. Fine.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary; by Mr. THOMPSON at the Gardens of the Horticultural Society at Chiswick, near London; and by Mr. VEALL at Boston.

Days of Month. 1837. March.	Barometer.				Thermometer.						Wind.			Rain.			Dew-point.
	London: Roy. Soc. 9 A.M.	Chiswick.		Boston. 8½ A.M.	London: Roy. Soc.			Chiswick.		Boston. 8½ A.M.	London: Roy. Soc. 9 A.M.	Chisw. 1 P.M.	Bost.	London: Roy. Soc. 9 A.M.	Chisw.	Boston.	Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
		Max.	Min.		Fahr. 9 A.M.	Self-registering. Min.	Self-registering. Max.	Max.	Min.								
W. 1.	30·355	30·431	30·421	29·99	34·7	32·0	42·2	38	31	35·5	NW.	NE.	calm	...	...	...	27
Th. 2.	30·331	30·394	30·237	29·94	35·2	33·7	38·6	42	30	33	NNE.	N.	calm	...	...	...	28
F. 3.	30·287	30·340	30·227	29·91	34·7	33·2	41·5	43	36	37	N.	N.	calm	...	...	...	30
S. 4.	30·027	30·134	30·106	29·71	41·6	34·8	42·3	45	32	42	WNW.	N.	NE.	·036	...	...	34
⊙ 5.	30·055	30·122	29·894	29·65	37·0	35·2	44·0	47	37	36	WSW.	NW.	calm	...	...	...	32
● M. 6.	29·926	30·028	29·990	29·60	40·3	36·8	44·8	42	34	38	N.	NE.	calm	...	...	...	34
T. 7.	30·049	30·161	30·102	29·65	38·7	37·0	42·6	47	28	37·5	N.	NE.	calm	...	...	...	33
W. 8.	30·193	30·245	30·157	29·79	37·7	35·4	44·8	49	37	35	SW.	NW.	calm	...	...	...	34
Th. 9.	30·057	30·124	29·917	29·53	42·8	37·7	47·6	49	38	42	SSW.	SW.	W.	...	...	...	38
F. 10.	29·599	29·685	29·323	29·13	45·3	40·8	49·2	50	33	43	S.	SW.	W.	...	·09	·06	40
S. 11.	29·265	29·319	29·301	28·87	41·7	37·7	47·0	50	26	37	S.	SW.	W.	·102	...	...	38
⊙ 12.	29·351	29·581	29·379	29·02	39·0	34·7	47·0	47	34	37	S.	SW.	calm	...	·09	...	35
M. 13.	29·791	30·253	29·877	29·61	36·3	35·2	44·2	46	32	35	N.	NE.	calm	·033	...	...	33
☽ T. 14.	30·322	30·378	30·306	30·05	38·3	33·8	43·6	45	34	37	N.	NE.	calm	...	...	...	34
W. 15.	30·160	30·223	30·114	29·90	38·3	36·7	44·2	41	36	37·5	E.	NE.	calm	...	·01	...	34
Th. 16.	30·044	30·205	30·104	29·75	38·4	37·2	40·6	40	36	39	N.	NE.	calm	...	·01	...	33
F. 17.	30·267	30·346	30·304	29·93	38·2	37·0	40·8	40	35	39	N.	E.	calm	...	...	...	33
S. 18.	30·201	30·275	30·142	29·94	38·8	36·2	41·6	43	29	37·5	N.	NE.	calm	...	...	...	33
⊙ 19.	30·019	30·099	29·987	29·69	35·7	31·3	41·6	41	30	39	N.	NE.	calm	...	...	...	31
M. 20.	29·873	29·977	29·906	29·55	36·3	32·7	41·2	40	24	33·5	N.	N.	N.	...	·10	...	31
T. 21.	29·861	29·930	29·707	29·51	30·8	26·7	39·0	37	27	30	WNW.	N.	calm	...	·04	...	25
○ W. 22.	29·765	29·839	29·776	29·46	31·0	29·5	35·0	40	23	33	E.	S.	calm	·102	·01	...	24
Th. 23.	29·710	29·764	29·730	29·46	32·7	28·5	37·0	40	21	31·5	NE.	SE.	calm	(*melted*	·01	...	23
F. 24.	29·734	29·844	29·784	29·55	31·8	26·8	37·3	40	27	27·5	N.	NE.	calm	*snow.*)	...	...	24
S. 25.	29·790	29·849	29·781	29·44	32·6	30·6	36·7	44	31	33	WSW.	NW.	calm	...	·02	...	28
⊙ 26.	29·703	29·869	29·756	29·44	38·6	32·4	42·2	49	24	39	SW.	NW.	calm	...	·06	...	32
M. 27.	29·981	30·077	30·033	29·70	34·3	27·8	43·4	44	32	30	SW.	W.	calm	...	·10	...	31
☾ T. 28.	29·940	30·013	29·868	29·51	39·3	34·0	40·4	49	36	38	SW.	W.	calm	...	...	...	31
W. 29.	29·679	29·755	29·716	29·33	43·4	38·8	47·4	49	34	41	SW.	SW.	calm	·066	...	...	36
Th. 31.	29·751	29·839	29·785	29·40	38·4	36·7	49·0	48	25	34·5	E.	NE.	calm	·094	...	·35	29
F. 30.	29·880	29·954	29·936	29·55	36·6	30·9	44·0	48	27	36	N.	NE.	calm	...	...	...	30
	29·934	30·034	29·924	29·59	37·4	33·9	42·6	44·2	30·9	36·2				Sum 0·433	0·54	0·41	31·5

THE

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[THIRD SERIES.]

JUNE 1837.

LXXX. *Experiments on the peculiar Voltaic Condition of Iron as excited by Peroxide of Lead; in a Letter to* Mr. Faraday. *By Professor* SCHŒNBEIN.*

DEAR SIR,

I ONCE more take the liberty to address to you by writing a short account of the results of my latest researches on the peculiar condition of iron. In my opinion, these results, though they do not yet solve the riddle of the subject, are such as to excite scientific curiosity, at least as much as the facts did, a description of which I had the honour to communicate to you last year. The space allotted to a letter being so small, I am obliged to be as concise as possible in describing the phænomena recently observed by me; but if you should be interested with the details of the subject, I take the liberty of referring you to a paper of mine which will hereafter be published in Poggendorff's *Annalen*. In the first place I must tell you, that the most powerful voltaic association into which iron can be brought in order to excite its peculiar condition, is that with peroxide of lead. A common iron wire, one of the ends of which is covered with this substance, proves to be inactive, not only towards nitric acid of a given strength, but towards nitric acid containing any quantity of water; whilst, as you know, my oxidized iron wire, or one associated with platina, &c., is acted upon by that acid if much diluted just in the same manner as unprotected iron. But the superiority of the association mentioned to any other at present known is exhibited in a still more striking manner by putting the two ends of an iron wire (one of which is covered by peroxide of lead)

* Communicated by Mr. Faraday.

into an aqueous solution of the common sulphate of copper, in the same way as the two ends of the oxidized wire, &c. are plunged into common nitric acid. Under these circumstances not the smallest particle of copper will be precipitated on any part of the wire immersed in the said solution. This peculiar state of the wire, however, lasts only so long as both ends of it are in the solution; for no sooner is the protected one removed from the liquid, than the other left immersed turns active, that is to say, throws down copper. In this respect, therefore, there is a great difference between the action of the wire in question upon the solution of blue vitriol, and that of the oxidized one upon common nitric acid. This difference of action implies another, namely, the impossibility of transferring, within the copper solution, the peculiar state from wire to wire, which it is so easy to do in nitric acid. I must not omit here to state the remarkable fact, that by mixing the solution of the sulphate with a comparatively small quantity of chloride of sodium (common salt) the calling forth of the peculiar condition is prevented, not only in the foregoing case, but in all that will be mentioned hereafter. This fact is by no means an insulated one, and depends upon the same cause as that which prevents the disengagement of oxygen at the iron (whilst constituting the positive electrode of the pile) from a solution of haloid salts, &c. Presuming that by rendering iron inactive towards sulphate of copper in the way described, a current would be excited the same as to its direction with that produced by calling forth the peculiar state of this metal within nitric acid, and having had recourse to the galvanometer, I was very much struck on finding that the needle was not in the least affected. The instrument I made use of in my experiments, though indicating rather weak currents, certainly does not possess the highest degree of sensibility possible, (it contains about 100 coils,) but as in a scientific point of view it is of very great importance to know whether the peculiar condition of iron can in any way be called forth without exciting a current at the same time, I beg you to decide this question by means of your most delicate galvanometer. If your experiments should happen to place beyond doubt the absence of any current under the before-mentioned circumstance, such a result would justify the deduction of very curious inferences from it, and prove in the first place that the inactivity of iron has, as to its origin, nothing to do with what we call a current.

A series of phænomena depending upon the action of iron wire (associated with peroxide of lead) upon a solution of sulphate of copper may be called forth, which exhibits a beautiful analogy to that set of facts communicated to you in my letter,

which you had the kindness to have inserted in the Philosophical Magazine, No. 59. (present volume, p. 133.) To obtain with iron in the said solution results similar to those mentioned in my letter, depending upon the action of this metal on nitric acid, the following conditions must be fulfilled. In the first and second case the oxidized iron wire E F is to be replaced by a wire, whose end E is covered with peroxide of lead: everything else remains the same as stated in my letter. As to the third fact, C P D is to be an iron wire having its end D associated with peroxide of lead; E F is to be a common iron wire. With respect to the fourth case, E instead of being oxidized must be covered with peroxide of lead. To obtain a result analogous to the fifth fact, the iron wire C P D, its end D being associated with the substance alluded to, must first be put into the vessels. Supposing D to be within B, the end E of an iron wire E F is to be plunged into A, and F afterwards into B. As the third case shows, F will turn under these circumstances inactive. Things being in this state, put the one end of a third common iron wire into B, and afterwards its other end into A, and F will cease to be in its peculiar condition. Now whatever the number of wires similar to that E F may be, all their inactive ends being within B turn active under the circumstances mentioned, though they do not touch each other anywhere. Concerning the sixth case, it is obtained exactly in the same manner as stated in my letter, provided the oxidized end be replaced by one covered with peroxide of lead. Bending up the common end of the experimental wire, is, however, not required. The best way of associating an iron wire with peroxide of lead is, to make it the positive electrode of a *couronne des tasses* (containing about a dozen of pairs of copper and zinc), and to put the free end of this wire into a solution of the common acetate of lead (*saccharum Saturni*) for about 8 or 10 minutes. By the action of the pile the peroxide is deposited on the positive iron wire.

En passant, I must tell you, that many reasons lead me to believe, that iron associated with this substance will form the most powerful voltaic element known, and I am just about to construct a pile of such couples. As to the chemical nature of the matter producing the colours of Nobili's chromatic scale* a notice of mine will shortly be published in Poggendorff's Annals, from which you will see that your idea upon the subject is entirely correct†, and that consequently the view of the Italian

* See Scientific Memoirs, part i. page 108.

† Mr. Faraday's opinion upon this subject will be found in the present volume, p. 176.—Edit.

philosopher wrong. Some of the facts regarding the peculiar condition of iron, and observed by Mr. Noad, are, as you will easily perceive, quite the same as those which were already stated in my letter above mentioned. In publishing them as new, that gentleman was most likely not aware of my observations.

I cannot close these lines without expressing you my sincere thanks for the service which you so kindly rendered me by forwarding my last paper to the Editors of the Philosophical Magazine. I am, my dear Sir, very truly yours,

Bâle, April 27, 1837. C. F. Schœnbein.

LXXXI. *Further Experiments on the peculiar Voltaic Condition of Iron as excited by Peroxide of Lead.* *By Professor* Schœnbein.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

As Mr. Faraday has perhaps communicated to you a letter of mine lately addressed to him, in which some new facts regarding the peculiar condition of iron are stated, you will lay me under many obligations to you by inserting in your next publication the following remarks. In the letter alluded to, I stated that by plunging an iron wire associated with peroxide of lead into a solution of common sulphate of copper no current is produced, or rather, that by means of my galvanometer I had not been able to discover one. Having since made the instrument more delicate, I succeeded the other day in tracing a current under the circumstances mentioned, and as it may easily be supposed, a current which originates in one part of the wire being not covered with peroxide of lead. From this fact, and those stated in my letter to Mr. Faraday, it appears in the first place, that iron combined with the last-named substance and put within a solution of blue vitriol gives rise to a set of phænomena, which in every respect are similar to those occasioned by an oxidized or platinized iron wire within nitric acid; and in the second place, that the peculiar condition of iron cannot be called forth by means of voltaic associations, without exciting at the same time a current, to which the iron turned inactive bears the relation of the anode. Now from such an invariable concurrence of two phænomena, I think we may safely infer that one of them is the cause of the other; and some facts render it probable that the inactivity of iron is the effect of a current, though we do not yet know in the least in what manner the first is occasioned by the latter. If some very weighty reasons did

not militate against the correctness of the idea, it might be imagined that the connexion between the two phænomena in question depends upon the fact, that the current issuing from the inactive iron carries along with it or keeps at a certain distance from the metal, those particles of the fluid which are round the metallic substance, preventing by this means immediate contact, and consequently chemical action. This idea receives some support from the fact, that an inactive iron wire is to a solution of blue vitriol, as to capillary action, what a solid body with a greasy surface is to water, whilst such is not the case with active iron. But the single fact that this metal can for any length of time remain inactive in nitric acid, under circumstances which exclude the possibility of the existence of a current, overthrows at once the above-mentioned hypothesis, not to mention many other facts, equally irreconcilable with it.

By the relation of iron associated with peroxide of lead to nitric acid and to the solution of blue vitriol, as well as by the fact that iron in this combined state excites a very strong taste upon the tongue, a taste much stronger than that produced by any voltaic association known, I was led to suppose that a powerful battery might be constructed of pairs consisting of iron and the said peroxide. Experiments have proved the correctness of my supposition: an iron wire $0'''\cdot5$ thick, $3''$ of length, one of its ends coated with a thin film of peroxide, and each end put into a separate vessel filled with nitric acid a hundred times diluted, developed a current which was capable of decomposing water. For when the two vessels were connected with a platina wire, hydrogen was evolved at one of its ends, oxygen at the other. At the extremity of the platina wire, it being placed in the vessel where the peroxide of lead was, the latter gas was disengaged. Twenty-four such little wires arranged into a *couronne des tasses* and the before-mentioned diluted nitric acid used as the exciting liquid, caused a current of considerable intensity; for it rapidly decomposed water only slightly acidulated and produced likewise a sensible shock. But as may easily be foreseen, such a pile is not active for a great length of time; for the hydrogen evolving at the negative part of each wire, that is to say, at the end covered with peroxide of lead, rapidly decomposes this substance, thereby reducing the wire to its ordinary state. Any other method attempted by me for the purpose of associating the peroxide with iron than that mentioned in my last letter to Mr. Faraday proved unsuccessful, though it is possible by attaching a small quantity of peroxide to a wire in a mechanical manner, to render this inactive in common nitric acid.

As to the facts observed by Mr. Noad, you will easily perceive that some of them have been already mentioned in my letter to Mr. Faraday, published in Number 59 of your Magazine. The conclusion drawn by Mr. Noad from some of his experiments, that iron being in its peculiar state is incapable of conducting current electricity, is, I am afraid, not admissible; for as I have shown elsewhere, an inactive iron wire can perfectly well perform the function of the positive electrode, even of a very small pile, without undergoing any change with regard to its peculiar condition; and besides this fact, there are many others, which do not allow the adoption of Mr. Noad's conclusion. It is, however, true, that iron in its peculiar state obstructs very much the passage of currents of low intensity, and acts in this respect very like platina.

I am, Gentlemen, yours, &c.

Bâle, May 8, 1837. C. F. Schœnbein.

P.S. You will oblige me very much by letting Mr. Faraday have a sight of the preceding letter previously to its insertion in your valuable Journal. S.

LXXXII. *On the Protochloride and Terchloride of Iodine.* *By* Robert Kane, *M.D., M.R.I.A.**

In the *Journal de Pharmacie* for February 1837, received here (at Dublin) April 4th, there is a paper by Soubeiran in which he describes a chloride of iodine, consisting of three atoms of chlorine and one of iodine, as new, and as having been first discovered by him. In the number of the Dublin Journal of Medical and Chemical Science for July 1833, I described this very body, as well as a lower chloride which appears to have escaped Soubeiran's notice; and as that memoir has evidently not attracted the attention of chemists, I take the liberty of subjoining the results contained in it in as brief a form as admits of their being intelligibly described. The difference of dates (four years nearly) renders it unnecessary to enter into any argument about priority.

"In order to obtain a compound containing the greatest possible quantity of iodine, I passed a current of chlorine through water in which iodine was diffused, leaving a considerable excess of iodine. The liquor became of a deep brownish-red colour; gave off fumes of chloride of iodine highly irritant to the eyes and nose; had a peculiar smell intermediate between those of its constituents; first reddened and

* Communicated by the Author.

then bleached litmus paper: when cooled considerably it deposited a considerable quantity of a reddish yellow matter, which was again redissolved by heat."

For analysis an excess of pure potash was added, the whole dried and ignited, redissolved and precipitated by nitrate of silver, the mixed chloride and iodide of silver separated by ammonia. In two analyses were obtained,

	1.	2.	Mean
Chlorine ...	22·36	23·76	23·06
Iodine	77·64	76·24	76·94
	100·00	100·00	100·00

The body Cl + I should give

Chlorine..............	35·42	21·9
Iodine..................	126·30	78·1
	161·72	100·0

" The solution of this chloride in water is deep reddish yellow. On the skin it produces a deep yellow mark, and smarting is not soon washed off. When heated it is partially decomposed, and by frequent distillations can be completely decomposed into iodine and the terchloride.

" When put in contact with the red oxide of mercury, the red or brown oxide of lead, or the oxides of copper, there is oxygen copiously disengaged, while chloride and iodide of the respective metals are produced, and some iodine deposited. With oxide of zinc this reaction is particularly remarkable. The action of this chloride of iodine on metallic chlorides gives rise to some interesting phænomena, which are exhibited in a tabular form in the original paper, to which I shall refer for the details. With protochloride of tin, the protochloride of iodine gives perchloride of tin and protiodide of tin in splendid orange prisms; iodine being separated by the first action but subsequently dissolved. Thus,

$$3\,(Cl + St) + (Cl + I) = 2\,(Cl_2 + St) + (I + St);$$

and with other protochlorides the metal is carried to the highest degree of combination with the chlorine."

" *Of the Terchloride of Iodine.*—When a solution of the protochloride of iodine has been repeatedly distilled, the quantity of iodine which separates each time diminishes, until at last a liquor is obtained which is vaporized unaltered. It then contains terchloride, although it is exceedingly difficult to obtain it pure. It can likewise be obtained by adding to protochloride of iodine a strong solution of corrosive sublimate, which throws down much iodine; and on pouring off

the clear liquor and distilling, the terchloride can be gotten nearly pure. A great number of analyses were made of it, which, though the specimens prepared in different ways and at different times gave results slightly varying, all agreed within narrow limits with the formula 3 Cl + I, which gives

Chlorine...	35·42 × 3 =	106·26	45·66
Iodine......	126·30 =	126·30	54·34
		232·56	100·00

"The properties of this compound, admitting its existence, are, generally speaking, similar to those of the protochloride, with one exception, which serves to distinguish it from it, and to determine when it is rendered impure by admixture with any of it. When the terchloride of iodine is mixed with protochloride of tin, iodine is thrown down; but when more chloride of tin is added, in place of forming the orange-red crystals of protiodide of tin with the excess of protochloride of tin, the iodine dissolves and forms a perfectly colourless solution; therefore, in distilling the chloride of iodine, as long as the distilled liquor forms any orange-red crystals with an excess of protochloride of tin, it has not been as yet freed sufficiently from protochloride of iodine."

Soubeiran doubts altogether the existence of the perchloride ($Cl_5 + I$), in consequence of his not having been able to obtain it pure. I have not myself made any experiments on that subject, but from the positive results of Davy and Gay-Lussac, I am still disposed to admit of its existence and to consider that there are three chlorides of iodine, Cl I, Cl_3 I, and Cl_5 I.

I cannot conclude this note without expressing the great pleasure I received from seeing my early results confirmed by the accurate experiments of Soubeiran, and in stating that although obliged by justice to myself to call the attention of chemists to my former paper, yet that the recent memoir in the *Journal de Pharmacie* has filled up an important vacancy in science, by showing that the terchloride was of a more permanent nature and could be obtained by simpler processes than I had been inclined to suppose; and I am sure had Soubeiran attempted to analyse the compound with maximum of iodine, he would have much illustrated the history of that body, which I was able at the time but partially to explore.

Dublin, April 12, 1837.

LXXXIII. *On the Thermo-electric Currents developed between Metals and Fused Salts.* *By* Thomas Andrews, *M.D., Professor of Chemistry in the Royal Belfast Institution.**

THE interesting discovery made by Faraday of the high conducting power of certain fused salts for voltaic electricity, led me to expect that electrical currents might be produced by bringing them into contact with the metals, analogous to the thermo-electric currents observed by Seebeck. Having easily succeeded in verifying this conjecture, and having observed that the currents thus produced exhibited some remarkable properties, I submitted them to a careful examination, the result of which forms the subject of the present paper.

To detect the presence of the electrical current, a very delicate galvanometer, constructed for me by M. Gourjon of Paris, was employed, in which the copper wire made nearly 3000 revolutions round the lower needle, and the system of needles was rendered as perfectly astatic as possible. A galvanometer having 20 or 30 coils, with astatic needles, will be found, however, sufficiently sensible to give decided indications of the passage of the principal currents which I shall have occasion to describe.

Having taken two similar wires of platina (such as are used in experiments with the blowpipe), and connected them with the extremities of the copper wire of the galvanometer that has just been described, I fused a small globule of borax in the flame of a spirit-lamp, on the free extremity of one of the platina wires, and introducing the free extremity of the other wire into the flame, I brought the latter, raised to a higher temperature than the former, into contact with the fused globule; the needle of the instrument was instantly driven with great violence to the limit of the scale. The direction of the current, as indicated by the deflection of the needle, was from the hotter platina wire through the fused salt to the colder wire. A permanent electrical current in the same direction was obtained, by simply fusing the globule between the two wires, and applying the flame of the lamp in such a manner that, at the points of contact with the fused salt, the wires were at different temperatures.

To discover whether the current had sufficient intensity to pass through acidulated water, a column of water (to which a few drops of sulphuric acid had been added), whose length was about half an inch, was interposed in the course of the

* Communicated by the Author.

circuit, the connecting poles in the water being formed of platina wires. On fusing the globule as before, the needle of the galvanometer was still deflected through an arc of 80° or 90°, but with less violence than when a complete metallic circuit was employed. When carbonate of soda was substituted for borax in these experiments, similar but more powerful currents were obtained.

My first attempts to obtain chemical decompositions by means of these currents were unsuccessful when the common forms of apparatus were used; but by employing poles exposing unequal surfaces, this object was finally attained*. A piece of bibulous paper, exposing on each side a surface of one fourth of a square inch, was moistened with a solution of

* The influence of the surface of the poles, in rendering perceptible the separation of the elements of an electrolyte, is very remarkable. Faraday has observed that not a bubble of gas will appear on the surface of a pair of platina plates, immersed in dilute sulphuric acid, when made the poles of a voltaic combination, formed by a single pair of platina and zinc plates charged with the same dilute acid; and hence that distinguished philosopher has inferred, that the tension of such a current is too low to effect the decomposition of water. On repeating and varying the conditions of this experiment, I found that if two fine wires were substituted for the platina plates the same negative result was obtained; but that if a platina plate exposing an extensive surface to the liquid was used as one pole, and a fine wire of the same metal as the other, then a minute stream of bubbles of gas arose from the wire, which after continuing for some time finally ceased to appear. An additional quantity of gas was, however, easily procured, either by increasing the surface of the broad pole, or by removing it and heating it to redness, or by reversing the direction of the current. The following appears to be a satisfactory explanation of these results. When the poles exposed on both sides equal surfaces, the gases were dissolved in the nascent state by the surrounding liquid; but when the polar surfaces were unequal, the solution of the gas being greatly facilitated by the broader pole, the element of the water separated there was dissolved, while the other element was disengaged, in the gaseous state, at the wire which served as the opposite pole. Indeed, Becquerel had already correctly inferred, from the circumstance of the plates acquiring polarity, that the water in this experiment of Faraday must have been decomposed. It is from the obstacle presented to the passage of the current by the acquired polarity of the platina plate, that the gas soon ceases to be formed in greater abundance than it can be dissolved by the water; and its reappearance under the circumstances stated before, is an obvious consequence from the well-known properties of polarized plates. By employing a similar artifice, a solution of sulphate of soda may be decomposed by means of a single couple of platina and zinc plates, charged with a solution of chloride of sodium, and the presence of the free acid or alkali rendered evident by its action on litmus or turmeric paper. In order therefore to discover, in case of difficulty, whether an electrical current is capable of decomposing water or other substances, it is necessary to employ poles having very unequal surfaces; and this will be effected in the most perfect manner by opposing a thick wire or plate of platina to one of Wollaston's guarded points.

the iodide of potassium, and laid on a platina plate, which was in metallic connection with one of the platina wires used in the previous experiments. The extremity of the other platina wire in contact with the globule, was applied to the surface of the bibulous paper, and the flame of the lamp was so directed, that the latter was the colder of the wires between which the globule of borax or carbonate of soda was fused. The platina plate in this arrangement therefore constituted the negative pole, and the extremity of the wire applied to the bibulous paper, the positive pole. Accordingly, when the circuit was completed, an abundant deposition of iodine occurred beneath the platina wire. When a similar wire of platina was substituted for the plate on the negative side, the effect was either none or scarcely perceptible.

A compound arrangement was next formed by placing a series of platina wires on supports, in the same horizontal line, and fusing between their adjacent extremities small globules of borax. The globules and wires were exactly similar to those that are used in blowpipe experiments. A spirit-lamp was applied to each globule, so as to heat unequally the wires in contact with it; and the corresponding extremity of each wire being preserved at the higher temperature, the current was transmitted in the same direction through the whole series. By connecting the extremities of four cells of this arrangement with an apparatus for decomposing water, in which the opposite poles consisted of a thick platina wire and a guarded platina point (both being immersed in dilute sulphuric acid), very minute bubbles of gas soon appeared at the guarded point, and slowly separating from it ascended through the liquid. They were obtained in whichever direction the current was passed, but rather more abundantly when the point was negative and the wire positive. With only two cells, similar bubbles formed in a visible manner on the guarded point, but in such exceedingly small quantity that they did not separate from it. With an arrangement containing 20 cells, a doubtful sensation was communicated to the tongue when the poles were applied to it; but no spark was visible, although the current was passed through a helix of copper wire surrounding a bar of iron, and the contact was broken with great rapidity by means of a revolving apparatus. It is necessary to observe, however, that the lamps were unprotected, and that it was impossible to render the flames of such a number of spirit-lamps burning near each other, so steady as to heat at the same moment, in the required manner, all the globules and wires. With an enlarged and more perfect form of apparatus, there can be little doubt that a spark might be obtained.

The extremities of the platina wires which were introduced into the globules of borax, after having been employed in these experiments, did not exhibit any appearance of chemical action; their lustre was untarnished, and their edges presented a sharp and well-defined outline, without being in the least degree rounded away. To render still more certain the absence of any chemical action, a very fine wire of platina was used as the hottest wire, in contact with the fused borax, and the circuit being completed by a metallic wire, a continuous current was maintained for several hours; but there was no apparent change either in the wires or the borax. With carbonate of soda instead of borax, the result was the same. When it is remembered that this current, if transmitted through a solution of the iodide of potassium, (in which case the greater part of the current is even interrupted,) would have produced in a few seconds a very perceptible deposition of iodine, it is impossible to imagine that the same current could continue, for a long space of time, to be produced from chemical action in one of the platina wires without any sensible alteration of the metallic surface. Besides, it is well known that under ordinary circumstances there is no chemical action exercised by platina upon fused borax or carbonate of soda.

It is certainly very interesting to see powerful chemical affinities thus overcome by simply bringing two metallic wires, at different temperatures, into contact with a fused salt, between which and the wires no [chemical] action takes place. The direction of the current is not influenced by the quantity of surface in contact with the wires, but depends altogether on the difference of temperature, as was ascertained by careful experiments.

Similar results were obtained when other fused salts were substituted for borax, such as carbonate of potash, chloride and iodide of potassium, sulphate of soda, chloride of strontium, &c. Even with boracic acid, which Faraday has observed to be a very imperfect conductor of voltaic electricity, I succeeded in deflecting the needle of the galvanometer through an arc of 40°, the circuit being closed by metallic wires. The direction of the current was the same as with borax.

To compare the intensity of these currents with those produced by chemical action, the galvanometer and a hydro-electric couple were both interposed in the course of the circuit, and the connections were so adjusted, that the currents developed by the fused salt and in the voltaic cell should be in opposition to each other. In this case the deflection of the needle would indicate the current of superior intensity. On comparing these currents with various hydro-electric com-

binations, they appeared, when fully developed, to have a somewhat superior tension to the currents produced by a couple of platina and silver plates immersed in dilute sulphuric or nitric acid. If the nitric acid was so strong as to dissolve rapidly the silver, then the voltaic current became superior.

The effect of substituting other metals for one or both of the platina wires still remained to be examined; but here considerable difficulties often arose, from the fusibility and tendency to oxidation of many of the metals.

When the platina wires were replaced by wires of palladium, currents in every respéct similar were obtained.

When platina was opposed to palladium, gold or silver, fused soda or borax being interposed, the current was always from the platina through the fused salt to the other metal, provided the platina was at a higher temperature. When the palladium was hotter than the platina, the current was reversed, or from the palladium to the platina. It was difficult to expose the gold or silver wire to a higher temperature than the platina without fusing it, when a globule of soda or borax was used; but by substituting a more fusible globule, formed of a mixture of the carbonates of soda and potash, the current was readily obtained from the silver or gold to the platina, so long as the former metals were maintained at a higher temperature.

These experiments prove that the position of the metals in the thermo-electric scale does not exercise any influence upon the direction of the current, which is altogether determined by the relative temperatures of the wires.

When platina at a higher temperature was opposed to copper, fused borax or soda being interposed, the current in very numerous trials (with one or two rare exceptions) was from the platina through the salt to the copper. It was only when from the action of the flame a very rapid formation and solution of the oxide of copper occurred, that the reverse current was obtained; but when the chemical action was not considerable the current was always from the platina. A current was also obtained in the same direction with boracic acid instead of borax. These results are the more interesting, as they prove most distinctly that chemical action cannot be the source of these currents, since in this example the platina would require to have been the metal attacked.

On substituting iron for copper a violent chemical action took place, the borax or soda became dark and opake from dissolving the oxide of iron, and the direction of the current was in general from the iron to the platina, even when the

latter was at a much higher temperature than the former. However, by fusing a small globule of borax or soda on an iron wire in the reducing part of the flame, and bringing a hot platina wire into contact with it, I obtained a current from the platina to the iron; but the experiment is difficult to perform and will rarely succeed.

When platina was opposed to the following metals, viz. antimony, lead, zinc, and tin, it was with some difficulty that even a mixture of the alkaline carbonates was maintained in a state of complete fusion, the platina being at a red heat, while the other metal was itself almost at the point of melting: the current was in every case from the platina through the fused salt to the other metal. In these cases it was evidently impossible to reverse the temperature of the metals. When the interposed globule consisted of chlorate of potash, the current was always from the oxidable metal to the platina, but here the chemical action was very considerable. In the case of the noble metals, the direction of the current was the same with the chlorate of potash as with the other fused salts.

It appears from the preceding experiments, that an electrical current is always produced when a fused salt capable of conducting electricity is brought into contact with two metals at different temperatures; and that when chemical action does not interfere, the direction of the current is not influenced by the nature of the salt or metal, being always from the hotter metal through the fused salt to the colder metal. This current has an intensity inferior to that of the hydro-electric current developed by platina and zinc plates, but greatly superior to that of the common thermo-electric currents, and is capable of decomposing with great facility water and other electrolytes. The source of this current may probably be simply referred to the contact between the heated metal and fused salt, which appears to be capable of generating an electrical current, more intense as the temperature of the point of contact is more elevated. According to this view, opposite currents are developed at the point of contact of each metal with the fused salts; but that which is produced at the point whose temperature is higher, having a superior intensity, overcomes the other, and its effects alone are exhibited; just as happens when two similar metallic junctions in a closed metallic circuit are exposed to unequal temperatures. The superior intensity of this current to those obtained from the metals alone, depends probably on the greater obstacle presented to the reunion of the two electricities, at the junctions where they are separated, from the inferior conducting power of the fused salt.

Hitherto I have only described the currents produced when the interposed salt is in a state of perfect fusion, but before the salt becomes actually fused, electrical currents are developed, whose direction no longer follows the simple law, that has been before enunciated, but varies in the most singular and perplexing manner. After a long and tedious investigation, I have been completely baffled in my attempts to discover the essential conditions upon which the directions of these currents depend, and I shall therefore describe at present only one or two experiments which will show the complicated nature of the inquiry, and may, perhaps, draw the attention of others to this curious part of electrical science. In the investigation of these currents a very sensible galvanometer must be employed.

A small platina spoon was partly filled with fused carbonate of soda, and the end of a thick wire of the same metal was introduced into the fused salt, metallic contact being carefully avoided. When the salt had cooled, the wire and spoon were connected with the galvanometer. On applying a very gentle heat to the bottom of the spoon, by means of a small spirit-flame placed at a considerable distance, a current was obtained from the spoon to the wire, or from the hot metal to the cold; this current was very feeble and could rarely be maintained beyond a few minutes. By increasing the temperature of the lower part of the spoon till the salt in contact with it entered into fusion, while the portion surrounding the cold wire was still in a solid state, a powerful current was obtained from the wire to the cup, or from the cold metal to the hot. When the temperature of the cup was still further raised so as to fuse the whole of the salt, the current was of course again reversed, being from the hot metal to the cold. It was interesting to observe the violent manner in which from this cause the needle of the instrument started from one extremity of its scale to the opposite, on the slightest movement of the flame.

To the class of partially fused salts belongs heated glass, which accordingly presented similar changes in the direction of the current. Thus when a platina wire was covered with a very thin coating of glass, and another wire at a higher temperature brought into contact with the glass, the current was from the cold metal through the glass to the hot. If a thicker piece of glass was interposed, the first current was from the hot wire to the cold, but on raising the temperature a current was obtained in the opposite direction. M. Becquerel had already observed by means of a sensible gold-leaf electroscope that when platina wires at unequal temperatures are separated

by means of heated glass, they exhibit signs of free electricity, one of them being connected with the ground and the other with the electroscope; but the general conclusion which he attempts to deduce from the result of this single experiment is certainly inaccurate, as it is founded on the assumption that the colder wire will give always signs of positive electricity, which we have seen is only true when the glass is thick and at a certain temperature. The conditions however here stated are not the only circumstances which influence the direction of the electrical currents with heated glass, but as my experiments do not lead to any definite result, I refrain from describing them.

These currents may likewise be obtained by interposing certain minerals between unequally heated wires; thus mica placed between platina wires and heated very strongly caused a deflection in the galvanometer needle of 7°, and the mineral called stilbite of 25°; the current in both cases was from the hot platina to the cold.

Belfast, April 11, 1837.

LXXXIV. *Descriptions of some new British Species of Hymenopterous Insects.* *By* J. O. Westwood, *F.L.S. &c.**

Encyrtus Dalmanni.

Capite thoraceque læte aureo- vel cœruleo-viridibus sericeis, scutello obscuriori; antennis thorace brevioribus sensim incrassatis, articulis 3—8, apice 9vi 10mi et 11mi omninò albidis; alis anticis nigricantibus, costâ versus et pone medium obscuriori, basi, maculis duabus marginalibus oppositis fasciâque subapicali albis; abdomine chalybeio; femoribus nigricantibus; tibiis tarsisque anticis luteis, summo apice fusco, tibiis intermediis flavis basi nigris, tibiis posticis nigris, tarsis pallidè luteis. Species pulcherrima. —Long. corp. $\frac{3}{4}$ lin. Expans. alar. $1\frac{1}{2}$ lin.

Habitat in quercetis com. Oxon., Cantian. et Surrej. Tempore æstatis. In mus. nostr.

Encyrtus Zetterstedtii.

Præcedenti valdè affinis. Capite thoraceque lætè viridi-cœruleis, sericeis; abdomine nigro-chalybeio; antennis nigris, articulis 7 et 8 albidis, summo apice fuscescenti; alis anticis nigricantibus, suturâ obscuriori, basi, maculâ oblongâ ante medium alterisque 4 oppositis, 2, 2, marginalibus albis; pedibus nigris; femoribus et tibiarum basi pedum anticorum nigris, tibiarum apice tarsisque fulvis, tibiis intermediis et tarsis flavis illorum basi nigris, tibiis posticis nigris summo apice luteo, tarsisque luteis.—Long. corp. $\frac{1}{2}$—$\frac{3}{4}$ lin. Expans. alar. 1—$1\frac{1}{2}$ lin.

Habitat cum præcedente. In mus. nostr.

Encyrtus albipes.

Thorace viridi-æneo tenuissimè punctato; capite viridi, fronte viridi-aureâ; antennis ferè longitudine corporis, luteis, pilosis, filiformibus, scapo albo-flavescenti; scutello cupreo, laminis pleurarum violaceis, pedibus gra-

* Communicated by the Author.

cilibus albo-flavescentibus, tarsorum apicibus fuscis; abdomine nigro-æneo; alis omninò hyalinis. ♂.

Encyrto zephyrino Dalm. valdè affinis.—Long. corp. $\frac{3}{4}$ lin. Expans. alar. $1\frac{2}{3}$ lin.

Individua 22 e larvâ *Tortricis* cujusdam folios involutos *Tiliæ* habitantis exclusa. Dom. Ingall. In mus. nostr.

Encyrtus sulphureus.

Pallidè sulphureus; oculis, antennarum clavâ tarsorumque apice fusco. Abdomine sessile, thorace majori ovato depresso apice subattenuato, alarum nervis pallidis, antennis capite cum thorace haud longioribus, versus apicem paulo et sensim incrassatis.—Long. corp. $\frac{1}{2}$ lin. Expans. alar. 1 lin.

Habitat in gramineis in Richmond Park. Fine Augusti 1833. In mus. nostr.

Encyrtus Schœnherri.

Luteus, sericeus; antennis fuscis, filiformibus, scapo ovali compresso albido, fasciâ latâ nigrâ; apicibus albidis; facie fulvâ; metathorace obscuro, abdomine parvo, conico, depresso; pedibus luteis; alis hyalinis immaculatis.—Long. corp. $\frac{3}{4}$ lin. Expans. alar. $1\frac{1}{2}$ lin.

Habitat prope Cantabrigiam, Windsor et Hampstead Heath, mense Julio. In mus. nostr.

Encyrtus Dahlbomii.

Thorace æneo tenuissimè punctato; abdomine nigro orbiculato depresso; facie fulvâ, vertice nigro; antennis sensim clavatis nigris, scapo latissimo compresso subtriangulari, articulis tribus ultimis (arctissimè conjunctis) albis; pedibus luteo-rufis; femoribus tibiisque posticis obscuris; alis fasciâ latâ mediâ fuscâ.—Long. corp. 1 lin. Expans. alar. $1\frac{3}{4}$ lin.

Habitat in gramineis apud Swanscombe, com. Cantian., Julio 1835. In mus. nostr.

Encyrtus Bohemanni.

Obscurè niger; capite thoraceque pubescentibus; abdomine oblongo-ovato depresso piceo; tegulis marginibusque scutelli luteis; antennis longioribus ferè filiformibus, scapo maximo compresso ferè orbiculari, articulis ultimis albis; pedibus luteis, posticis paullò obscurioribus, alis immaculatis.—Long. corp. $\frac{5}{6}$ lin. Exp. alar. $1\frac{2}{3}$ lin.

Habitat in gramineis apud Richmond Park, com. Surrej, Julio 1835. In mus. nostr.

Encyrtus hederaceus.

Albo-canescens; antennis pallidè fuscis sensim crassioribus, apice albo; facie luteâ capite thoraceque testaceis, hoc griseo squamoso, collari albido; abdomine parvo orbiculato depresso fusco; pedibus fusco-albidis; tibiis posticis obscurioribus.

Affinis *E. punctipedi.*—Long. corp. $\frac{1}{2}$ lin. Expans. alar. 1 lin.

Habitat in Hederâ, Chiswick. Julio 1834. In mus. nostr.

De genere Choreio (*Choreia* Westw. Mag. Nat. Hist., vol. vi.; *Crantor* Hal. Ent. Mag., No. iv. p. 268.)

Genus olim characteribus e fœminâ desumptis institutum, nunc characteribus masculinis emendo.

Corpus ♂ breve crassum apterum ut in fœmina. Differt præcipuè antennis masculinis corpore toto paulò longioribus filiformibus 11-articulatis, scapo brevi; articulo 2do brevi, 3tio sequentibus longiori 4—8 æqualibus, brevioribus, tribus ultimis arctissimè conjunctis. Reliquis cum fœmina convenit.

Typus *Choreius ineptus.* ♂. ♀.

Nigro-æneus; antennis piceo-rufis, basi apiceque nigris; pedibus piceis; tibiarum apice tarsisque rufis.

Choreia nigro-ænea. Westw. loc. cit.

Encyrtus ineptus. Dalm. Esenb. Hym. Mon. 2. 255.

Sphenolepis inepta. Esenb. loc. cit. p. 258.

Obs. 1. Genus *Sphenolepis* Esenbeckii, omninò distinctum nec ad subfamiliam *Encyrtides* pertinet.

Obs. 2. Individua nonnulla fœminea cum speciminibus apteris cepi, alas quatuor perfectas nigricantes possidentia. Hæc pro specie diversa non considero; sed potiùs individua evolutionem perfectiorem gaudentia; speciminibus alatis *Veliæ currentis* analoga. Nihil inter insecta Hymenoptera huic simile adhuc observatum est.

AGONIONEURUS ALBIDUS, Westw.

Totus pallidè flavescenti-albidus; oculis obscuris; alis immaculatis albidis, longè ciliatis, callositate stigmaticali magìs conspicuâ quàm in *Ag. basalii*, angulum parvum cum margine alarum formanti, antennarum articulo 5to præcedenti multò majori.—Long. corp. $\frac{1}{3}$ lin. Expans. alar. $\frac{5}{6}$ lin.

Habitat —? In mus. nostr.

AGONIONEURUS SUBFLAVESCENS, Westw.

Totus pallidissimè flavescens; oculis ocellisque paulò obscurioribus; alis subhyalinis, flavido vix tinctis callositate stigmaticali quàm in *Ag. albido* minùs distinctâ, antennarum articulo 3, 4 et 5 æqualibus.

Obs. Insectum vix conspicuum.—Long. corp. $\frac{1}{3}$ lin. Expans. alar. 1 lin. In mus. nostr.

Habitat in sepibus apud Sylvam Coombe, æstate 1835.

LXXXV. *Some Account of the Art of Painting in Enamel. By Mr.* ALFRED ESSEX.*

THE perusal of the excellent paper on Glass-painting which appeared in the Philosophical Magazine for December 1836, having revived an idea which I had formerly entertained of drawing up for publication a brief view of the allied art of PAINTING IN ENAMEL, I have now endeavoured to bring my intention into effect. This art is mentioned in some former papers in the Magazine, but as it is usually introduced merely incidentally, some further account may not be unacceptable to the present readers of this work.

Before proceeding to my more immediate subject, allow me to make a remark or two on that of painting on glass.

Mr. J. T. Cooper observes, in a paper on "the Composition of the Ancient Ruby Glass †," that "the chief difference between the ancient and modern ruby glass ...... consists in the hardness, or infusibility of the basis on which it is flashed, that which is now manufactured being of flint, while the former is

* Communicated by the Author.

† Annals of Philosophy, Second Series, vol. vii. p. 105.

of the hardest crown glass; also the difficulty of obtaining it of any size, and free from cloudiness or opacity." These objections, I understand, apply equally to that which is made at the present time*; to which it may be added that the modern ruby glass is inferior to the ancient in this respect also, that while the latter, on being exposed to the heat of a glass kiln, preserves its colour unimpaired, that of the former suffers considerable injury by such exposure, in some cases becoming almost black. The importance of this difference will be duly estimated when it is considered that, in consequence, the modern ruby cannot be painted upon, as the heat required to fix the fresh colour would destroy the beauty of its original appearance. To meet this difficulty the modern artist has recourse to the following ingenious expedient. He paints upon a piece of plain glass the tints and shadows necessary for blending the rich ruby glow with the other parts of his picture, leaving those parts untouched where he wishes the ruby to appear in undiminished brilliancy, and fixes the ruby glass in the picture behind the painted piece. Thus in such parts the window is "double-glazed."

Your "Correspondent" says that "the material employed by the old glass-makers to tinge their glass red was the protoxide of copper;" but it would appear from the analysis made by Mr. Cooper that the colouring material was not copper alone, for he states that he obtained in the process "a copious precipitate of chloride of silver."

It is generally believed, as stated also by the author of the paper on "Glass-painting," that copper yields the green in enamel-painting. This statement is true if it is confined to the productions of those artists who practised painting in enamel prior to the late Mr. Charles Muss. He employed, as I do, the oxide of chromium to produce this colour, and discarded copper altogether: in the composition of enamel colours, I entirely reject also the use of iron and manganese.

In the paper on Glass-painting now referred to, it is observed that "the accounts to be found in various works respecting this curious art are by no means satisfactory or complete:" this observation may be extended, without offering the least violation to truth, to the equally curious and beautiful art of PAINTING IN ENAMEL. Writers on the subject of enamelling confound the art of painting *in* enamel, with those of painting *on* glass and porcelain, although these three arts are almost as dissimilar as their products,—a painted window, a richly ornamented vase, and an enamel painting.

Enamel is a substance having for its basis a white and per-

* Mr. Cooper's paper appeared in 1824.

fectly transparent glass. When a small quantity of oxide either of gold, silver, cobalt, copper, or some few others of the metals is added to this base it produces a coloured transparent enamel. This enamel is used on silver and gold, and is applied to the ornamenting of snuff-boxes, watch-cases, and various articles of jewellery. Previously to the application of the enamel, various patterns and devices are *bright-cut* in the metal with the graver or the rose-engine, and the cuts, reflecting the rays of light from their bright and numerous surfaces, exhibit through the richly coloured enamels a beautiful and gorgeous play of colours sparkling in varied forms with every change of aspect. Sometimes this enamelled *bijouterie* is further adorned with paintings in enamel, executed on rich transparent grounds, when, in some instances, a sunlike splendour is imparted to the whole scene by the rays of the engine-turned gold shooting from behind the mountains in a landscape, or diverging from the bosom of a lake. The enamel which, when painted upon, produces the most agreeable effect in these applications, is that which is opalescent, and which by enamellers is called *opal*; the soft cream-coloured and fiery appearance of the gem being imparted in this imitation by the oxide of arsenic.

When oxide of tin or of antimony is added to the transparent base mentioned above, the result is an opake enamel. I suspect, but am not certain, that oxide of antimony enters into the composition of some of the Venetian enamels. I have made an enamel with it alone, as the colouring matter, whiter than some specimens of foreign manufacture, and having in a high degree the waxlike appearance formerly so much valued by the makers of enamel clock- and watch-dial plates. But oxide of tin is certainly the substance to which opake enamel commonly owes its opacity and whiteness*.

The enamel used for making the plates upon which paintings in enamel are executed is imported from Venice. It is in the form of round cakes, varying in size from three to seven inches in diameter, and from half to three quarters of an inch in thickness, and weighing from half a pound to three pounds each. It is cream-coloured, heavy, less brittle than glass, is sufficiently hard to scratch crown-glass; its fracture is conchoidal and exhibits a resinous lustre, and it fuses at a temperature a little below that which will melt gold. Its com-

* There is a substance made at the glass-houses near London, the commercial name of which is *glass-enamel*, that owes its measure of opacity and whiteness to the oxide of arsenic. It is very glassy, brittle, easily scratched, readily fusible, and very white. It is used for making the common kinds of clock- and watch-dials and the white semi-opake ornaments for the mantel-shelf, toilet, &c.

mercial value varies from 12 to 20 shillings per pound. I have not analysed it, but its constituents, as stated by various authors, are silica, an alkali, and the oxides of lead and tin, and, I suspect, as before observed, oxide of antimony also.

An *enamel colour* is, like enamel, composed of a colourless and perfectly transparent glass for its base and owes its colour to a metallic oxide. Thus silica, borax, and the red oxide of lead form a base or *flux* for some colours. The habitudes of the oxides require that each should be treated with reference to its peculiar qualities, for instance, the flux which when employed with gold is best adapted for the production of a beautiful colour, is inefficient if used with the oxide of cobalt.

The plates for paintings are prepared thus: a plate of gold, or more usually of copper*, is covered with three successive layers of enamel, the enamel having previously been ground in an agate mortar; each layer requiring to be passed through the fire and melted before the next is laid upon it†. The plate being thus prepared, the artist proceeds in painting the picture in a similar manner to the painter in oil or in water colours, accordingly as the subject may require. The principal difference is this, that instead of waiting for the colours to dry before proceeding to lay on another coat, the painter in enamel has his work passed through the fire. By this process the colours are completely vitrified, and are incorporated with the body of the plate. This is not so completely the case with paintings on glass and on porcelain. The colours on these usually adhere only to the surface, and, under some circumstances, they are known to chip off‡. Glass and porcelain, also, do not admit of being subjected to so high a temperature as enamel plates, and hence the colours for painting on those substances are manufactured to melt at a much lower degree of heat than those used by painters in enamel. This

* The French and other Encyclopædias state, that silver is used for this purpose; and Walpole, in his "Anecdotes of Painting, &c." says that Petitot used plates of silver. This cannot be correct, for silver has the property of cracking the enamel in all directions every time it is passed through the fire, and hence it becomes necessary to expose plates of that metal when enamelled, to a sharp heat, in order to flow the enamel, that the cracks may close. This it is obvious would effectually destroy the drawing of a picture if it did no other injury. Silver is therefore only used for transparent enamelling, but in this application it is not so rich and beautiful as gold, and is only employed when the high value of gold is an object of consideration, as in the silver stars which are worn by the members of certain orders of knighthood, masonic emblems, military ornaments, &c.

† For a particular account of the manipulations practised by the enameller, see the article ENAMELLING in Rees's Cyclopædia. In this article the details are minutely faithful, though with reference to dial-plates modern improvements have rendered obsolete most of the processes described in it.

‡ See Brongniart "On the colours obtained from the metallic oxides," &c. Phil. Mag. First Series, vol. xiii. p. 342 *et seq.*

property of easy fusion is obtained by the introduction of a larger proportion of oxide of lead or of alkali, or of both, into the composition of the colours; which superabundance renders the flux of the colours an *imperfect* glass, and consequently lays it open to decomposition, from the attacks of those gases, which, being continually evolved from putrescent and other substances, are ever floating in the atmosphere.

The difficulty of working the colours with delicacy, and the extreme care required in effecting this, render the process of painting in enamel slow, and hence it has seldom been applied with success to painting from life, but has usually been employed in copying*. Indeed its permanency obviously points out, as perhaps its most legitimate use, the transmission to posterity of faithful transcripts of those eminent works which time is daily injuring and is certain ultimately to destroy. To effect this object no other branch of art appears competent. Engraving is adequate to transmit light and shadow, design and drawing, but colouring is wholly unattainable by it. But how much of the beauty and merit of a fine work of art is dependent upon its beauty of colouring! Nor can the richness and sweetness of a good colourist be attained either on glass or on porcelain, the chemical action induced by these substances, when at a high temperature, being inimical to really good colouring, while that of enamel, on the contrary, tends to impart depth and sweetness to every tint. Another advantage possessed by enamel over glass and porcelain is worthy of notice, and this is, that while the latter do not admit of being subjected to the fire more than from three to five times, the former knows no other limit than the finish of the picture. Paintings in enamel are usually passed through the fire ten or twelve times, and indeed sometimes oftener. This unlimited application of his efforts affords to the artist the opportunity of imparting to his work the finish of a Gerard Douw and a Mieris, and also of attaining with precision the deep, rich, and sweet tones which are seen in the productions of Correggio, of Guido, of Rubens, and of Reynolds.

To obtain the richness of the master-colourists it is obviously necessary that the painter in enamel should be in possession of colours capable of emulating those of the painters in oil. In this however the artists of former times were sadly deficient†. But, fortunately for this durable and beautiful art,

* Walpole states of Petitot, that "His custom was to have a painter to draw the likeness in oil, from which he made his sketches, and then finished them from the life."

† Dr. Ure in his Chemical Dictionary gives, from the Transactions of the Society of Arts, what he terms "A valuable list of receipts for enamel colours." The unfortunate artist who shall attempt to make colours for the

the discoveries of modern chemistry have afforded the materials to supply this long-sought desideratum. From three of the metals which till lately were known but to chemists, and which were regarded as curiosities only, namely, platinum, uranium, and chromium, are already produced four of the richest and most useful of the colours on the palette of the painter in enamel. And doubtless we may look to this source for the means for further improvement. Before the introduction of oxide of platinum a positive rich brown was unknown in enamel *: this colour when produced by the mixture of others, as was previously the practice, was liable to alteration by repeated fires, becoming more opake and meagre, and acquiring somewhat the appearance of common brown clay. With such a material how was it possible for an artist to obtain that deep, rich, and juicy transparency which is so highly and justly valued by every judge of painting, and which distinguishes the works of the great masters both ancient and modern? The oxide of platinum on the contrary yields a beautiful, indestructible, and richly transparent enamel brown, which no intensity or frequent application of the furnace can injure.

Mr. Cooper observes † that with the black oxide of platinum "we can now produce an enamel colour which preserves an intense black in the lighter shades, and is, moreover, capable of sustaining the most violent fire, without injury, which none of the former colours [blacks] will bear, without change." On this I must remark that I have made many experiments with this oxide, but have never been able to produce with it an intense black enamel colour. A black it certainly will produce, but not of sufficient intensity to be useful to the painter. I have a black of great intensity which is unchangeable in the fire, and into the composition of which the black oxide of platinum does not enter. I have exposed this colour to the heat of an enamelling furnace about forty times without any apparent alteration of its tint or diminution of its intensity.

Colours proper for painting in enamel are not to be purchased: those sold for the purpose are adapted only for painting on china. I have devoted much time to their improve-

purpose of painting in enamel from these receipts will assuredly find, to his disappointment, that they are utterly useless. The statements made in books upon vitrifiable colours are really unaccountable, and truly does M. Brongniart observe in his essay, that "it is very remarkable, that if the processes described in these works were strictly followed, it would never be possible to form the colours for which they pretend to give recipes;" and M. Clouet is justified in exclaiming as he does of the authors, "None of them say what they ought respecting enamel." (Phil. Mag., First Series, vol. vii. p. 3.)

* For this invaluable acquisition the enamel painter is indebted to the late talented and indefatigable Mr. Muss.

† Journal of the Royal Institution, vol. iii. p. 121.

ment for the use of my brother Mr. William Essex, Painter in Enamel to H. R. H. the Princess Augusta. One of the objects which I have endeavoured to accomplish, and in which I have not been unsuccessful, is, that they should be of the same colour when on the palette as they will be when they have passed through the fire. The colours possessing this property, the artist is enabled to see *while proceeding with his work*, the precise effect that will be produced after the painting has undergone fusion. Thus the power of attaining greater precision in imitating the original is secured.

In Brongniart's "Essay on the colours obtained from the metallic oxides and fixed by fusion on different vitreous bodies," which has been before quoted, it is observed that oxides "which adhere little to the great quantity of oxygen they contain, cannot be employed.......The colour they present cannot be depended on, since they must lose it in the slightest heat by losing a part of their oxygen." This assertion looks very well in theory, but I confess I was surprised to find such a statement put forth by an able practitioner. In his paper on the black oxide of platinum*, Mr. Cooper observes, "A curious property of this oxide should here be mentioned. When heated *per se*, or with combustibles, it is easily reduced, but when mixed with enameller's flux, it is capable of sustaining a very intense heat, without decomposition; indeed it has withstood reduction in the most violent degree of heat I was able to give it." To this may be added, that no colours are more to be depended upon, more indestructible in the fire, than those prepared from the oxides of platinum and of gold; and yet of the oxides of these metals it may be said, in the language of M. Brongniart, that they above all others "adhere little to the oxygen they contain," they standing lowest among the metals for affinity for oxygen†.

Every person at all acquainted with the receipts for enamels, as framed by those who had not that light to guide them which is afforded by modern chemistry, must be aware of the strange jumble which they almost universally present. Feeling certain that here, as in every other instance in which excellence is sought, simplicity was desirable, I have kept the attainment of

* Journal of the Royal Institution, vol. iii. p. 121.

† See a paper by V. Regnault in the Number for August, 1836, of the *Annales de Chimie et de Physique*, which appears to give the latest results on this subject. I am aware that it is conceived by Prof. Proust and others that the gold in the powder of Cassius, (which is employed to produce a purple colour in enamel,) is not in the state of oxide. Various considerations, however, have led me to a different conclusion, and I am much pleased to find that I am supported in this opinion by authority so eminent as that of the late Dr. Turner. See his Elements of Chemistry, Fifth Edition, p. 645.

this in view throughout the experiments which I have made for the purpose of obtaining a set of enamel colours which should combine with the required richness of tone and brilliancy of tint, the property of remaining unchanged during the numerous exposures to the heat of the furnace to which a painting in enamel is necessarily subjected. Of the necessity for simplification, and also of the extent to which it is capable of being carried, take the following example.

In vol. xxxv. of the Transactions of the Society for the Encouragement of Arts, &c. p. 49, it is stated, that "Twenty guineas were this session (1817) voted to Mr. R. Wynn for receipts for enamel colours and for staining and gilding on glass." One of these receipts, for green, is given thus, at p. 60:

"A frit for transparent greens.

"Take flint powder	3
Flux No. 2 (p. 55)	3
Green pot-metal glass	$1\frac{1}{2}$
Red-lead	$7\frac{1}{2}$
Raw borax	$2\frac{1}{2}$
Green oxide of copper	$1\frac{1}{4}$

"Melt them in a crucible, pour out the mass, and pound it in an earthen mortar.

Take of the green frit	3
Of yellow enamel colour (p. 56)	$1\frac{1}{2}$

"If too soft add Naples yellow."

Now in order to see the whole complexity of this receipt let us make an analysis of its contents; and then contrast it with the simplicity which experience shows to be attainable.

Frit.

- Flint powder 3
- Flux, No. 2. 3
  - Flint glass. 10
    - Silica
    - Potash
    - Ox. of lead
  - White arsenic 1
  - Nitre 1
- Green pot-metal glass. .... $1\frac{1}{2}$
  - Silica
  - Alkali
  - Oxide of lead
  - Oxide of copper
- Red-lead .... $7\frac{1}{2}$
- Raw borax .... $2\frac{1}{2}$
- Green ox. of copper .... $1\frac{1}{4}$

Of this when melted take 3

- Of Yellow Enamel Colour*. .... $1\frac{1}{2}$
  - 1
    - Red-lead 8
    - Oxide of antimony 1
    - White oxide of tin 1
  - Flux, No. 4. .... $1\frac{1}{2}$
    - Red-lead .... $9\frac{1}{2}$
    - Borax .... $5\frac{1}{2}$
    - Flint glass. .... $1\frac{1}{2}$
      - Silica
      - Potash
      - Ox. of lead

Here it may be observed that silica is introduced in four

* This receipt for "yellow enamel colour" is given by Mr. Wynn at p. 56 of the volume referred to.

different instances, and oxide of lead in six, and except one instance of introduction of the former substance, and two of the latter, the artist must of necessity be ignorant of the proportions in which they exist in the artificial compounds which he employs. Foreign substances also are present, such as iron, manganese, &c., which, although in minute quantities, are injurious and create confusion. These are not noticed in the foregoing analysis, because their introduction or omission, as likewise their proportions when introduced, are dependent upon accident and the pleasure of the manufacturer.

Let us now proceed to contrast this complex process with the result which some attention to the progress of chemical science has enabled the enameller of the present day to arrive at. The following are at once the materials and substantially the constituents of the green enamel colour which Mr. W. Essex has in use:

Silica.
Borax.
Oxide of lead.
Oxide of chromium.

Here the simplicity is such that all the substances which enter into the composition of the colour are known to the maker, and the proportions in which they shall exist are entirely within his command.

The enamelling-furnace, in which the smaller plates are prepared and the smaller paintings also *fired*, is a square space of about twelve inches in height, in depth, and in width, surrounded by solid brickwork, and opening into a vertical flue in which is a register for regulating the heat. It is elevated a convenient height from the ground, and has an iron plate hearth in front for the purpose of holding the plates and paintings both before and after they have passed through the fire. The bottom of the furnace, when prepared for use, is covered to about three inches in thickness with coke *, upon which the muffle is placed. The muffle has neither bottom nor back, and is surrounded

* The old enamel painters had a notion that no fuel but charcoal was suitable for an enamelling furnace. The late Mr. Hone held this opinion, and the late Mr. Grimaldi frequently had fires made with charcoal alone for his paintings, because, as was imagined, the colours "came out better;" but I never could discover that those paintings which were treated with charcoal displayed any superiority over those which were fired with coke. In conjunction with Mr. Muss I made several experiments to test the truth of this notion, and these proved it to be fallacious. Coke is by much the more convenient substance, as its combustion is slower, and consequently the heat can be maintained without interruption for a longer time by its means than with charcoal, than which it is also very much cheaper.

with coke except in front. An iron door, having an aperture in it the size of the front of the muffle, closes the whole. The entire draught of air supplying the furnace passes through the muffle. The plates and paintings are placed on thin slabs, made of tempered fire clay, technically termed *planches.* When the fire has burnt up sufficiently, the plate or painting, after having been dried by being placed on the iron plate opposite the fire, is gradually introduced under the muffle, the planch resting on the bed of coke. The greatest heat, it is obvious, will exist at the back of the muffle; it is necessary therefore that the picture should be turned while in the fire that it may be heated equally over its entire surface; this is effected by means of a pair of spring tongs. When the colours are seen to be properly melted the painting is withdrawn and placed on the iron hearth to cool. In this furnace plates are prepared and paintings fired from the smallest size up to about five inches in diameter; but for larger works a furnace of a different construction is required. The muffle of the large furnace has a bottom and a back, and its mouth is closed also by a door made either of iron or of fire-stone. From the circumstance of its thus being closed on all sides it has acquired the appropriate appellation of a *close muffle*, that before described being termed, in contradistinction, an *open* muffle; the essential difference being that while the entire draught of the furnace passes through the latter, it is wholly excluded from the former. In the large furnace the fire is placed under the muffle only, and is supported by iron grate-bars, the construction, in fact, closely resembling that of a common air-furnace. The draught passes between the bars and carries the flame into the flue, which commencing at the top of one of the sides of the fire-place, conducts it over the muffle, which it leaves, by means of flues constructed in the same plane with its bottom, on the side opposite to that at which it enters. The flame after enveloping the muffle plays against the bottom of an iron oven. This oven contains several shelves, and its use is, to *anneal* the paintings, this being necessary to prevent them from cracking when in the fire, which they would do if exposed suddenly to the heat of the muffle. The furnace is so arranged that the bottom of the annealing oven becomes of a dull red heat at the time when the muffle attains the proper state for receiving the paintings, and this is indicated by its interior becoming of a glowing orange heat, the muffle itself having to sustain a heat nearly adequate to the fusion of cast-iron. By this arrangement the paintings, as they are placed in the annealing oven while it is cold, are gradually heated until they arrive at a temperature at which they can with safety endure the much

higher heat of the muffle. They are likewise returned to the oven after they have undergone superficial fusion in the muffle, it being requisite that their cooling also should be effected gradually.

Painting in enamel having been reproached in the hearing of Mr. Muss as a style of art in which neither texture nor crispness was attainable, because, as was alleged, the colours when in fusion would flow smooth and mingle, Mr. M., being conscious that enamel possessed the capabilities of the styles both of oil and water, determined practically to vindicate his art from the reproach. For this purpose he painted that unequalled production in enamel, the Greyhound, which now forms part of His Majesty's collection. The original, by J. Ward, R.A., is painted with all that bold crispness for which the works of that eminent artist are celebrated, and in the enamel this peculiarity is faithfully preserved. By what means Mr. Muss accomplished this, is not fully known; but my brother, when copying a picture by Sir David Wilkie, having occasion for crisp painting, I undertook some experiments with the view of furnishing him with colour possessing the required quality of melting soundly, but retaining at the same time the sharpness and precision of form with which it had been touched on. The result of these experiments was the production of colour which, though completely vitrified, will, if required, retain even the sharpness of a needle point. In fact, transparency, crispness, and texture, (as indeed my brother's works may evince,) are now equally attainable in enamel as in any other mode of painting.

The nature of the material and the expense attendant upon attempts to produce large works have tended to restrict the dimensions of enamel paintings. Until the time of the late Henry Bone, R.A., Painter in Enamel to His Majesty, but few attempts had been made to extend the size beyond that suitable for trinkets. That artist, with amazing perseverance and industry, overcame innumerable difficulties, and exhibited annually, for a long series of years, enamels of large dimensions. Petitot, whose works are usually minute, painted, it appears, a picture "9¾ inches high by 5¾ wide* regarded by Walpole as indubitably the most capital work in enamel in the world"; but in this attempt he seems not to have been quite successful, for "the enamel is not perfect in some trifling parts*": this picture is stated "to be in the collection of the Duke of Devonshire*." In the reign of Queen Anne an artist named Boit undertook a painting in enamel of the extraordinary size of

* Walpole's "Anecdotes of Painting."

"from 24 to 22 inches high by 16 to 18 wide." He, however, failed to produce the picture, after having received an advance of 1700*l*., and having expended upwards of 800*l*. in fruitless attempts to accomplish his object*. It appears, therefore, that the largest works which have been executed in enamel are, the Bacchus and Ariadne, after Titian, by Bone; and the Holy Family, after Parmegiano, by Muss.

Mr. Bone's picture measures 18 inches by 16½, and was painted after the original by Titian, now in the National Gallery. It was purchased of the artist by the late George Bowles, Esq., for 2200 guineas, and was subsequently in the possession of the Hon. Miss Rushout.

Mr. Muss's picture measures 20½ inches by 15¾ †, and was painted after the original by Parmegiano, in the possession of Sir Thomas Baring, Bart. Upon the decease of the artist it was purchased by His late Majesty George IV., for the sum of 1500 guineas, and now forms part of the Royal Collection at Buckingham Palace. This great work then, it would appear, is the largest painting in enamel that has hitherto been executed.

It may be assumed that in general painting in enamel is best adapted for pictures of smaller size, yet in some cases circumstances may exist which render it desirable that a painting should be perpetuated in an enamel of even larger dimensions than those just noticed, and in the present state of the art no insuperable difficulty exists to the accomplishment of such an object.

Whether, participating in the general fate of the productions of man, paintings in enamel will, in the lapse of ages, alter, fade, and resolve into their original elements, is a problem the solution of which must be left to future generations. Nevertheless their power of extreme duration is established by the fact that some rude specimens of vitrified colours have been found in Egypt, which have existed between two and three thousand years, but which still appear as fresh as if they were but the productions of yesterday‡. This power of resisting decay renders enamel a valuable medium for conveying down the stream of time the likenesses of celebrated individuals. Portraits, whether executed in oil or in water colours, change in a comparatively short period, the rosy tints becoming pale,

* Walpole's Anecdotes of Painting.

† The plate was made for Mr. Muss by the writer of this paper.

‡ It does not appear that the Egyptians practised enamelling on metal. Specimens of gold inlaid with enamel exist in the collection of Egyptian antiquities at the British Museum, but none in which the enamel has been vitrified on the metal.

and the high lights sallow; and where the delicately transparent tints shed their deepening beauty opacity gradually supervenes. But those which are fixed in enamel will carry on unchanged to a period indefinitely remote the most delicate as well as the richest of the tints originally imparted by the pencil of the artist; and as he left the portrait of the sage, the poet, the warrior, and the beauty, so will they remain, when even the marble which portrayed their forms or told their history may have crumbled into dust.

35, Northampton-street, Clerkenwell, March 1837.

LXXXVI. *On Hydrate of Magnesia. By* G. O. Rees, *M.D., F.G.S., &c.*

To Richard Phillips, Esq., F.R.S., &c.

Dear Sir,

SHOULD you think the following worthy of notice, pray favour me by inserting it in the Philosophical Magazine.

Your sincerely obliged,

G. O. Rees.

59, Guilford Street, Russell Square, May 16, 1837

It has been supposed by some chemists that magnesia is capable of uniting with water in several proportions, though no analysis seems to have been made of the artificial hydrate of that earth. The native hydrate of magnesia from America, analysed by Dr. Bruce, yielded in 100 parts,

Magnesia............	70
Water...............	30——100.

A specimen of the same mineral from Unst, analysed by Dr. Fyffe, yielded in 100 parts,

Magnesia............	69·75
Water...............	30·25——100.

The results of two analyses made by myself of the artificial hydrate agree very nearly with the proportions obtained by Dr. Fyffe from the native specimen. Thus in a first experiment 100 parts yielded,

Magnesia............	69·63
Water...............	30·37——100.

A second experiment gave,

Magnesia............	69·41
Water...............	30·59——100.

These specimens were prepared by digesting recently calcined magnesia in cold distilled water and then drying the

mixture over a water-bath. The first was digested in a well-closed vessel during fourteen days; but no greater combining proportion of water was observed than in the second specimen, which was only digested for twenty-four hours.—I have procured a similar result by merely moistening the earth, and immediately drying it over the water-bath. The combination is immediate, and, as has been shown, admits of no increase in the proportion of water by a prolonged digestion. If boiling water be used in forming the hydrate, no difference is observed in its constitution. It may be remarked how nearly the first analysis approaches to the proportions of atom to atom, for assuming 20·7 as the atomic weight of magnesia, we have,

Magnesia............	20·7
Water	9·02

It seems certain from these experiments, that magnesia is capable of combining with water in one proportion only.

The precipitate obtained by the addition of ammonia to a neutral solution of sulphate of magnesia approaches also very nearly in constitution to a protohydrate of the earth. I found 100 parts of the precipitate (well dried over a water-bath) to yield

Magnesia............	66·7
Water...............	33·3——100.

LXXXVII. *Replies by* Mr. E. M. Clarke, the Rev. Professor Callan, *and* Dr. Ritchie *to certain Papers on Subjects of Electricity and Magneto-electricity inserted in the preceding and present Volumes of the Philosophical Magazine.*

THE following is the substance of communications which we have received from the gentlemen above named. We have omitted nothing which could tend to elucidate the subjects under discussion: indeed the reply of the Rev. Professor Callan to Dr. Ritchie, and that of the latter to the Rev. J. W. MacGauley, are inserted almost entire.

Reply of Mr. E. M. Clarke *to* Mr. J. Saxton. (See vol. ix. p. 360).

"I proceed to reply to Mr. Saxton's remarks one by one. In the first place I have not laid claim to the *electro-magnetic machine* (as he calls it) as my invention, but I have certainly termed a magnetic electrical machine E. M. Clarke's, owing to a material difference having been made in its construction, the advantages arising from which I shall now proceed to point out. First, it will readily be admitted that vibration tends to injure the magnets. In Mr. Saxton's machine very great vibration is unavoidable, in as much as all the ma-

chinery is attached to the magnet: a glance at the figure in Mr. Saxton's paper will at once prove the truth of this assertion. Another disadvantage attending his instrument is, that the magnetic battery cannot be readily detached so as to be applied to other purposes. Then again comes the mercury cup with all its attendant trouble and loss of time, owing to the incessant scattering of the mercury, the necessary filtrations owing to its oxidations, &c.

"Let me now oppose to these evils the advantages which I may fairly claim as being possessed by my machine. First, the instrument can be worked without any possibility of vibration, and the magnetic battery can be withdrawn from the instrument with the greatest facility. I have now also succeeded in dispensing with mercury, for a description of the mode of doing which I beg leave to refer your readers to the second number of Mr. Sturgeon's 'Annals of Electricity, Magnetism and Chemistry, and Guardian of Experimental Science,' which also contains a description of my magnetic electrical machine. 'Mr. Clarke's machine,' says Mr. Saxton, 'differs from mine only *in a slight variation in the situation of its parts.*' Every scientific inquirer knows what an amazing difference a slight variation in the situation of parts will often occasion in a philosophical instrument. But to proceed, he continues to say that my instrument is in no respect superior to his. I have already pointed out a few facts to prove that it is superior, and shall, ere I conclude, convince your readers that it is in every respect to be preferred. All I can expect, all I ask for is fair play. Let the public witness *what* Mr. Saxton's machine can do, and *all* that it can do, next witness the effects produced by mine, and then let them decide which they prefer.

"I shall now proceed to another assértion of Mr. Saxton's, viz., that no description of his machine had been published previously to the insertion of his attack upon me. Why, a description appeared in the number of the Mechanics' Magazine for May the 3rd, 1834; again, in Sir Richard Phillips's work, printed in 1834; again, by Mrs. Somerville; and also in the Catalogue of the Adelaide-street Exhibition: what, then, is Mr. Saxton's meaning?

"Let us now examine a little more minutely into the real facts of the matter as connected with the construction of the machines. By Mr. Saxton's own showing (fig. 3) the cylinders A B, on which the wires for the shock are coiled, are of the same size as the cylinders C D, round which the short wires for giving the spark are coiled. My principle is totally different. Different as regards the size of the cylinders (those

for quantity being double the diameter of the intensity cylinders); different as respects the relative thickness of the wires (the intensity wire being $\frac{1}{96}$ of an inch in diameter and 1500 yards long, the quantity wire being $\frac{1}{18}$ inch in diameter and 40 yards long); different also in my armatures being separate, for were they to be in one piece, to develop the full effects of both quantity and intensity the poles of the magnetic battery should be separated more than three times the distance they now are by my having the armatures unconnected. And yet this is what Mr. Saxton would call "a *slight* variation in the situation of the parts"!

"I now come to speak of Mr. Saxton's statement respecting the quantity and thickness of the wire he uses; for I confess that I for one, on referring to his figures 3 and 4, cannot imagine that he has ever constructed an instrument of the kind he describes. It must be evident that the quantity of wire required for C D, would be so disproportionate to that necessary for A B, as to render necessary a greater space between the cylinders C D (containing the quantity of thick wire), and the cylinders A B, having a smaller quantity of thin wire.

"Again, Mr. Saxton speaks of *insulation*, saying that the front end of the spindle is for that purpose, being made of ivory or hard wood. Insulation of what? I use nothing of the kind; insulation being quite unnecessary. Will Mr. Saxton still maintain that my machine does not differ in principle also on this point? Next we come to his description of the arrangement for giving the spark. According to his description, if the blade F leaves the surface of the mercury at the moment when the cylinder C D is vertical, then A B being on the magnets, and the current in C D having been for some time neutralized, the proper point would be so far different from what he states, as, instead of being vertical, to form an angle of nearly 45°. 'For obtaining the shock,' says Mr. Saxton, 'the points should be removed,' &c. I answer, *Let any person try to obtain the shock with the points removed.*

"He omits to state to whom he is indebted for the suggestion of removing one of the points. But I shall take the liberty of doing so, and he will remember that the individual alluded to was Mr. Ellicott, assistant to Charles Payne, Esq., late the deservedly respected Superintendent of the Adelaide Gallery, who is perfectly ready and willing to prove the correctness of this statement. We now come to what may be called the historical portion of Mr. Saxton's paper, and here I would remark, that if I had written a history of magnetic machines, instead of merely describing my own, and had omitted to mention any one in particular, I then might have fairly been

accused of being disingenuous; but I have not proposed or attempted such a history.

"In Paris I was told by Professor Pouillet that there was as much difference between my machine and Newman's (meaning Saxton's) as there was between the latter and Pixii's. The same words nearly were used in London by Dr. D. B. Reid, Messrs. Sturgeon, Leithead, and Bachhoffner; and in Dublin the same remark was made by Professor Lloyd and Dr. Apjohn, the former of whom, although previously in possession of Mr. Saxton's machine, purchased one of mine, and the latter has actually entrusted to me one of Mr. Saxton's to be altered according to my principle of construction for the Royal College of Surgeons, Ireland. Mr. Saxton next proceeds to state that until December 1835, he had not added the double armature, whereas I sold three of my machines with double armatures in the April preceding.

"Mr. Saxton goes on to state that his first idea of the double armature suggested itself to him on his seeing Count di Predevalli's machine in November 1833. From the same source I derived my idea, but certainly before Mr. Saxton, in as much as I had the machine in my hands before he had ever seen it. I was then in the employment of Watkins and Hill, to whom this machine was sent on its coming from Paris to be put in order, and I was the person in whose hands it was placed. I attended it at the Gallery, and can assuredly state that the machine did not exhibit the effects stated by Mr. Saxton. It did not charge the Leyden jar. The electrometer, or rather the electroscope, was affected, because the jar had been previously charged with dry electricity, and the residual electricity was the cause of the effect attributed by Mr. Saxton to the magnetic machine. Again, if the idea occurred in 1833, why did he not put it in practice until 1835? The truth is, it was not until after he had seen my machines with the double armatures, and after he saw the effects produced by a dwarf machine of my making, under the directions of Charles Payne, Esq., deposited in the Adelaide Gallery, as contrasted with his gigantic instrument. I can, moreover, fearlessly call on Dr. Faraday to bear me out in my statement that he himself told me the machine as constructed by me gave more powerful shocks than any he had previously seen.

With respect to Mr. Saxton's observations of the cause of the difference between the states which are generally termed quantity and intensity, I beg leave to offer one or two remarks. He states that the investigations of Dr. Henry of Philadelphia, Mr. Jennings, and Dr. Faraday fully proved that the spark is best obtained from a magneto-electric coil when short!

Why, according to this, a person unacquainted with the science would expect to obtain a spark from a single coil! 'And that,' he continues, 'the shock is best when the coil is long.' My experiments do not bear out this assertion; my machine depends upon the thickness of the wire for the spark, and its tenuity for the shock, within, of course, certain limits; and as far as I have carried my investigations, with nine pounds weight of steel, magnetized precisely to that degree which it will retain under all circumstances save being made red hot, a wire $\frac{1}{18}$ inch thick and 15 yards long gives a certain amount of quantity effect, 20 yards of the same sized wire gives more, but 25 yards less; while, on the other hand, 700 yards of wire $\frac{1}{96}$ of an inch thick gives a certain degree of intensity, but 800 yards of the same size less. With respect to his assertion that I have no claim to the double armature, I shall not add another word, confident as I am that I have already fully answered him on that point, and in a way which I trust will be perfectly satisfactory both to him and the public.

"With respect to Mr. Saxton stating that my 'piracy' consisted not in manufacturing his instrument, but in suppressing all mention of his name as connected with it,—I certainly have not manufactured any of his instruments since my improved machine was perfected, although I have had to alter several, and I do not therefore see what right he has to expect mention to be made of his name.

"Of his last remark I cannot admit the truth, for so far from having appropriated to myself Ampère's *Bascule électrique*, in a paper of mine in Mr. Sturgeon's Annals, published a month before Mr. Saxton's attack, I acknowledged the priority of all similar contrivances of which I had any knowledge."

No. 11, Lowther Arcade, Feb. 15, 1837.

The Rev. N. J. Callan, *Professor of Natural Philosophy in the Roman Catholic College, Maynooth, in Reply to* Dr. Ritchie. (See Lond. and Edinb. Phil. Mag., vol. ix. p. 61.)

Dr. Ritchie says that the battery described by me has been known for a long time. May I ask Dr. Ritchie why he has not referred to some work where a description of such a battery is to be found? He next says that he "had one exactly the same, ... six or seven years ago. Dr. Ritchie neither describes his battery nor refers to any description of it; it is therefore impossible for me to point out the difference between it and the battery described by me in the Philosophical Magazine for December (vol. ix. p. 472). With the aid of an electro-magnet, and a small instrument for rapidly breaking communication between the battery and helix of an electro-magnet,

the battery described by me, though it contains but 20 pairs of plates, is capable of producing in the space of one minute 3000 or 4000 electric currents, each equal in point of intensity to that of a battery containing 1000 or 2000 voltaic circles. That the helix of an electro-magnet is capable of giving a shock at the moment when battery communication is broken, was discovered only about two years ago (Phil. Mag. for November 1834, p. 351); that the shock given by the magnetic helix increases with the number of plates employed was not known till discovered by me within the last year. Surely, then, Dr. Ritchie had not a battery six or seven years ago, which, though containing only a small number of plates, was capable of producing in the space of one minute 3000 or 4000 electric currents, each equal in point of intensity to that of a battery containing 1000 or 2000 voltaic circles.

Again, Dr. Ritchie says, "the author speaks of a shock as if it were a *quantity*, and institutes a comparison between the *size* of the shock and the number of plates." That I speak of the shock as if it were a quantity, and that I institute a comparison between the *size* of the shock and the number of plates, Dr. Ritchie infers from my saying that "with one pair of plates the shock from the helix of the electro-magnet was equal to that of a battery containing 20 pairs of plates; when two pairs of plates were used, the shock appeared to be doubled; with three voltaic circles it appeared to be treble; and with every increase in the number of voltaic circles, there appeared to be a proportional increase of the shock." Had Dr. Ritchie read the remainder of the paragraph from which these words are taken, he could not but see that his inference is most unjust. For in the following line I speak of the shock as strong, and never speak of it as if it were *large* or had *size*. The obvious meaning then of my words is, that the shock increases with the number of plates, not in *size*, but in strength or intensity. My language may be inaccurate; but it is the language of Mr. Singer, who, in his treatise on galvanism, speaks of the shock as *increasing*: it is also the language of Dr. Ritchie himself, who says that "the physiological effects (among which he of course includes the shock) continued to *increase*." (Phil. Mag., June 1836, p. 455.)

Lastly, Dr. Ritchie says, that "the only thing new in my paper is the affirmation that an electro-magnet, when its magnetism is induced by a compound battery of 200 small pairs of plates, will have a greater power of inducing magnetism at a distance than any permanent magnet." Dr. Ritchie adds, "the very looseness of this statement is a proof of its fallacy." The looseness of this statement is due to Dr. Ritchie himself,

who has substituted the indefinite article *an* for the definite article *the*. In my paper it is not affirmed than *an* electro-magnet, but that *the* electro-magnet, (that is, the electro-magnet of which I was speaking, or which was formerly used in the apparatus for continued rotation,) when its magnetism is induced by a battery of 200 pairs of small plates, will have a greater power of inducing magnetism at a distance than any permanent magnet;" that is, than any permanent magnet substituted for the electro-magnet the use of which has been abandoned. Dr. Ritchie asks, " does the author mean to say that a *small* electro-magnet when connected with a battery of 200 pairs of plates induces more magnetism on soft iron at a distance than *any* permanent magnet?" I mean to say, what my words clearly imply, that the electro-magnet formerly used in the apparatus for continued rotation, when rendered magnetical by the voltaic current from a battery of 200 pairs of plates, will have a greater power of inducing magnetism on soft iron at a distance than any permanent magnet substituted for the electro-magnet, or than any permanent magnet of equal or nearly equal size. And this position Dr. Ritchie has not disproved, nor will he be ever able to disprove.

But is there the slightest foundation for Dr. Ritchie's assertion that the only thing new in my paper is the affirmation that the " electro-magnet, when its magnetism is induced by a *compound* battery of 200 small pairs of plates, will have a greater power of inducing magnetism at a distance than any permanent magnet?" I answer that this assertion is utterly destitute of foundation.

In my paper it is proved, first, that the magnetic power given to an electro-magnet by the voltaic current, and the distance at which that power is exerted, increase nearly in proportion to the number of plates employed; secondly, that the shock given by the helix of an electro-magnet, when battery communication is broken, increases nearly in proportion to the number of plates employed; thirdly, that the shock given by a long wire, on breaking communication with the battery, increases with the number of plates in the battery; and fourthly, that with the aid of an electro-magnet, and of a small instrument for rapidly breaking communication between the battery and magnetic helix, a battery containing but 20 voltaic circles may be made to produce effects of decomposition equal to those of a battery containing 1000 or 2000 voltaic circles. Surely, were it in his power, Dr. Ritchie would not have failed to show by reference to some published works, that I was not the discoverer of the three above-stated facts, or of the means of rendering a battery of 20 pairs of plates as effective in pro-

ducing decomposition as a battery of 1000 or 2000 pairs of plates.

Maynooth College, Feb. 12, 1836.

Dr. Ritchie *in reply to the* Rev. J. W. MacGauley.

(See p. 130.)

Mr. MacGauley describes the results stated in my paper contained in the Phil. Mag. for June 1836, as "perfectly at variance with the truth." The following is his version of my statement: "It is a well-known fact, that we receive a more powerful shock when electricity is being induced on a body, than when the induced electricity is returning to its natural state." The quotation is quite correct,—except the substitution of the indefinite article *a*, for the definite article *the*, before the word *body*. This slight substitution makes "my talk very egregious nonsense." My original statement is, that the shock is most powerful when the electricity is being induced on the (*human*) *body*, and I may further add, that this is always the case, whether the shock result from a single wire which has been connected with the voltaic battery, or from the coil of an electro-magnet. My statement therefore has no reference whatever to the electric state of the wire, or in other words, to the question, whether the electricity of the wire is being *forced* from its state of equilibrium or *returning* to its natural state.

Mr. MacGauley has also mistaken the experiments of Dr. Faraday on the length of the coil influencing the electric spark. His experiments were made with a single wire, either in one continuous length or in the form of a coil, *with* a galvanic battery *alone*. The *shock* from a long wire has long since been obtained by Professor Henry of America. Mr. MacGauley speaks of an *electro-galvanic* helix as a species of electric machine of almost unlimited energy. There is nothing, however, gained by forming it into a helix, the single continuous wire of the same length being equally efficacious. The following quotation clearly shows that the author really knows nothing of Faraday's admirable investigations: "The assertion I do make is this, and I repeat it, that if the iron of an electro-magnet retain, from the nature of its material, the presence of a keeper, or any other cause, the magnetism induced upon it, the shock and spark will be proportionally diminished, because the magnetism of the bar, by its inductive action on the helix, would prevent the perfect restoration to equilibrium of the electricity disturbed in the helix, by giving to the bar in a greater or less degree the nature of a permanent magnet, from which, by means of a helix coiled around it, neither

shock nor spark can be obtained. Will any one deny this except Dr. Ritchie? I believe not."

Dr. Faraday's investigations have completely established the fact, that it is only *magnetism* (or the electricity which constitutes magnetism) in *motion* which can either *induce* electricity on a wire, or partially prevent the electricity already induced from returning to its natural state. A permanent magnet then *within* a coil, whilst it remains as such, *can have no influence whatever* in preventing the "perfect restoration to equilibrium of the electricity disturbed in the helix." It is quite true that a piece of soft iron, if it could be placed within a helix connected with a battery, without having magnetism induced on it, would partially weaken the returning electricity in the coil, by the *reaction* of the electricity induced on it by the returning current, and the more it *differs* from a *permanent* magnet the more would it act in *diminishing* the returning current.

The concluding part of the preceding quotation contains the following remark. Since neither shock nor spark can be obtained from a wire coiled round a permanent magnet, therefore a permanent magnet can *diminish* the spark and shock which the coil would of itself give when returning to its natural state. If Mr. MacGauley will look at a short paper of mine in this Number, he will find that both a *powerful* shock and a *brilliant* spark may be obtained from a *permanent* horseshoe magnet having a wire coiled round it.

Mr. MacGauley accuses me of mistaking his results because he uses a *galvanic* helix instead of a *magneto-electric* machine. Now if I have not always mentioned by name the galvanic helix, it was simply because the electricity induced on the conductor is exactly the same whether the inducing cause be a *voltaic* battery or a magneto-electric machine.

We have noticed in Poggendorff's *Annalen* some remarks bearing upon several of the controversial papers on subjects of electricity and magnetism that have appeared in our last and present volumes, which we feel that we ought not, in candour, to omit to mention. They occur in a note appended to an abstract in that Journal of the Rev. Professor Callan's paper on a new voltaic battery, inserted in our last volume, p. 472. M. Poggendorff observes, after referring his readers to a paper by Dr. Ritchie also in that volume, containing a statement controverted by Prof. Callan, "the same volume of this Journal [the Philosophical Magazine] contains several other papers on the subject of magneto-electricity, which, as proofs of the great interest taken in this branch of natural philosophy in En-

gland at the present time, are very agreeable, but the whole of which contribute very little to its advancement." He then notices Mr. Rainey's first paper (vol. ix. p. 72), stating that the remarkable fact which it describes had before been observed in America by Henry and Ten Eyck*, and had recently been placed in a clearer light by Prof. G. Magnus, in Pogg. *Ann.*, vol. xxxviii. p. 436. The subsequent discussion by Dr. Ritchie and Mr. Rainey is then noticed, and also Mr. Mullins's paper (vol. ix. p. 120) and Dr. Ritchie's comment upon it (*ib.* p. 222). (Poggendorff's *Annalen der Physik und Chemie*, vol. xxxix. p. 410.)

In the course of his abstract of Prof. Callan's paper, M. Poggendorff inquires what Prof. C. means by "attraction," in vol. ix. p. 477. (*Ib.*) He also notices Mr. Saxton's account of his instrument.

LXXXVIII. *Proceedings of Learned Societies.*

LINNÆAN SOCIETY.

May 24, 1837.—THIS day, being the Anniversary of the Linnæan Society, Edward Forster, Esq., V.P. and Treasurer, took the Chair, in the absence of His Grace the Duke of Somerset. Dr. Boott, the Secretary, having stated that the Society had lost, by death, fourteen Fellows and four Foreign Members, proceeded to particularize them as follows:

Rev. Sackville Bale, of Withyham, Sussex.—This venerable man had been a Fellow of the Society for forty-five years, and his life was passed amid the charities of religion and the peaceful pursuits of natural history. Edward Forster, Esq., one of the Vice-Presidents, and the Treasurer of our Society, in a letter to me speaks of him in the following terms: "My old friend was a very respectable Sussex clergyman, the associate of all the botanists of our younger days, and among them the venerable James Dickson. He was a zealous promoter of the study of natural history, though it is to be lamented he never published on the subject. His parsonage, close to Withyham church, was most beautifully situated, with a large piece of water in front about a furlong below, on which it was delightful to see many of the more rare species of birds sporting at liberty as if in their native haunts; while others, still more domesticated, were strutting up the house-steps and entering without fear to share with their kind master the family repast. Behind rose an excellent garden, well stocked with the scarcest plants, British and foreign, for botany was his favourite pursuit."

The Very Rev. Henry Beeke, D.D., Dean of Bristol.—A zealous English botanist, and frequently quoted in the pages of "English Botany."

Thomas Marquis of Bath, K.G., &c.—A nobleman liberally disposed to patronize the science of botany and to advance the interests

* See also Phil. Mag. and Annals, vol. x. p. 314.

of this Society. He was one of the most generous contributors to the fund for the purchase of the Linnæan Collections.

Edward Turner Bennett, Esq., Sec. Zoological Society.—In alluding to the death of Mr. Bennett I am strongly reminded of those painful feelings with which the intelligence was first received; for though his illness had excited alarm for a day or two in the minds of some of his friends, and especially of those who were immediately around him, it was generally unknown, so that the first intimation which most of us received of it was that it had terminated fatally. It was but a few days before that we had seen him in the enjoyment of his usual health; and notwithstanding the apparent delicacy of his constitution, from our having been accustomed to witness his untiring devotion to his favourite pursuits, we had naturally been disarmed of all idea that his useful life was so soon to be brought to a close.

I had not the honour of knowing him intimately, but it was impossible for any one who enjoyed even his casual acquaintance not to be impressed with his intelligence, the gentleness of his manners, and the unobtrusiveness of his character.

The cordial interest he took in his zoological studies, the kindness and the intelligence he displayed in answering the inquiries of others, his ardour in the promotion of zoology, the animated sense he had of the moral and intellectual enjoyment to be derived from it, the absence of all unworthy rivalry in his character, and the affectionate esteem he was held in by those who were intimately associated with him in his pursuits, were ample proofs of his excellence, and of how serious a loss we have sustained in him as the friend and the naturalist.

One of his intimate friends*, to whom I applied for some information respecting his writings, says to me: "I can scarcely trust myself to speak of him in the terms that naturally present themselves upon the recollection of all that was so good, so kind, and so talented in his character. I believe I never knew a man in whom was combined so much that was admirable and endearing. His duties as Secretary to the Zoological Society were performed with such zeal, talent, and extensive information as can never be forgotten by those who had the opportunity of watching his labours and of acting with him. His published works are not perhaps equivalent in importance to his deservedly high character as a naturalist. His knowledge of zoological literature was perhaps more extensive than that of any other person in this country."

The only paper which Mr. Bennett communicated to the Transactions of the Linnæan Society was "A notice of a peculiar property in a species of Echinus," which forms a nidus for itself by effecting a cavity in rocks off the coast of Clare in Ireland. His contributions to the Zoological Journal were numerous, and nearly all the analyses of zoological works contained in it were made by him. The Proceedings of the Zoological Society from their commencement, and the

* Thos. Bell, Esq., F.R.S.

first volume and the first part of the second volume of its Transactions, were edited by him, and he contributed a great number of scattered notices and many very valuable papers to them.

Of his separate works "The Tower Menagerie" appeared in 1829, and "The Gardens and Menagerie of the Zoological Society" in 1830 and 1831, and an edition of "White's Natural History of Selbourne," to which he added many interesting notes and illustrations, was published soon after his death.

These publications, the zealous discharge of the duties of secretary, first of the Zoological Club of the Linnæan Society and afterwards of the Zoological Society, with the more unobtrusive but not less useful services which he rendered to zoology by the advice and assistance which he afforded to all its cultivators who asked them at his hands, were his chief contributions to natural history. His intimate friends are fully aware how large a portion of his time and how much pains and labour he bestowed to the furtherance of the objects of others; and there are few of the zoologists of this country who would not bear testimony to the fact that by means of the assistance thus afforded he contributed to facilitate the progress of zoology in Great Britain and to give it its proper direction. He died in his 40th year, and has left behind him an enviable remembrance in the minds of many amongst us whose scientific attainments and moral worth deservedly place them high in our esteem.

Henry Thomas Colebrooke, Esq., F.R.S., &c.,—one of the most distinguished Oriental scholars of Europe, the successor of Sir William Jones as Judge of the Native Court in Bengal and as President of the Asiatic Society of Calcutta, and the founder of the Royal Asiatic Society of London; a man almost as eminent for his scientific ardour as his high literary attainments.

Botany and geology were among the most favourite of his pursuits. He contributed three papers to our Transactions:

"A description of select Indian Plants," in 1817.

"On the Indian Species of *Menispermum*," in 1819.

"On *Boswellia* and certain Indian *Terebinthaceæ*," in 1826.

Alexander Collie, Esq., Surgeon R.N.,—a native of Scotland and beloved by all who knew him for the kindness of his disposition. He accompanied Capt. Beechey on his voyage to Behring's Straits as surgeon, and made valuable collections in natural history. He went out with the first settlers to Swan River, and died at King George's Sound in December 1835, bequeathing to the Linnæan Society his collection of dried plants which he had made in that colony.

Mr. Edward Donovan,—author of various splendidly illustrated works on the zoology of this country and on the insects of India and New Holland. He wrote the articles Conchology and Entomology in Rees's Encyclopædia. His works perhaps exhibit more of the splendour of art than of any enlarged views of science. He added some species to the previously existing knowledge of detailed zoology; and it is painful to reflect that one who had laboured so much in the cause of science should not have escaped the penury that too often waits on age.

John Latham, M.D., F.R.S., &c.,—one of the original members of this Society, who for nearly half a century took the liveliest pleasure in its prosperity and advancement.

This venerable and amiable man devoted himself to his favourite science of ornithology with undiminished interest to the close of his long life, which was extended to his ninety-sixth year. His writings on ornithology were very voluminous and are essential to every student; for though his views are perhaps limited in some respects, compared to those of more modern authorities, he made important use of the labours of previous naturalists, and added many species to those formerly known.

His great works are

"*Index Ornithologicus*," in 2 vols. 4to, 1790; and "A General History of Birds," in 10 vols. 4to, 1821—1824.

He contributed three papers to our Transactions:

"On the various species of Sawfish," in 1793.

"Observations on the Spinning Limax," 1797.

"Essay on the Tracheæ of Birds," 1797.

It was a privilege of no ordinary kind to me, who had not attained by several years even the moiety of the age of this venerable man, to see him a few years ago, at our anniversary dinner, triumphant in body and mind over the assaults of time; and I remember looking upon him with reverence,—not exclusively that becoming respect ever due from youth to age, whatever may be its intellectual characteristics, but that mingled feeling which partly arose from the impressive consciousness that a life so protracted, and exhibiting so much calm assurance of happiness, such serenity and cheerfulness of feeling, in a scene from which so many of his early friends had gone for ever, bespoke a mind at peace with itself and the world, and afforded a lesson of what true enjoyment lies beyond even the Psalmist's limit to the age of man, when time appears to have forgotten the good man's claim to a better state of existence; and it was impossible not to feel that his pursuits of natural history had perhaps contributed largely to the complacency and the elasticity of his almost patriarchal age.

William Elford Leach, M.D., F.R.S.—Few men have ever devoted themselves to zoology with greater zeal than Dr. Leach, or attained at an early period of life a higher reputation at home and abroad as a profound naturalist. He was one of the most laborious and successful as well as one of the most universal cultivators of zoology which this country has ever produced.

His discoveries in the different classes of the Vertebrata, especially Birds, were extensive; but it was in Entomology and Malacology that his labours have been most known and his improvements of the greatest importance.

His knowledge of the Crustacea was superior to that of any other naturalist of his time, and his arrangement the best until the work of Dr. Milne Edwards appeared two years ago.

After a long suspension of his studies from ill health, during which and up to the period of his death he was attended by the most de-

voted of sisters, he returned to his favourite occupations with his habitual ardour, and the letters he wrote to his scientific friends in this country exhibited the same devotion to the study of nature which distinguished the brighter years of his life.

His principal work, "The Natural History of the Mollusca of Great Britain," in the possession of his friend Mr. Bell, is not yet published. His other works were:

"*Malacostraca Podophthalma Britanniæ*," 4to, 1815 and 1816, not finished.

"Zoological Miscellany," 3 vols. 8vo, 1817.

"On the Genera and Species of Proboscideous Insects," 8vo, 1817.

He described the animals taken by Cranch in the expedition of Capt. Tuckey to the Congo; and was the author of valuable articles in the Encyclopædia Britannica, Edinburgh Encyclopædia, Philosophical Transactions, Zoological Journal, Memoirs of the Wernerian Society, Dictionnaire des Sciences Naturelles.

Between 1810 and 1820 he contributed seven papers to the Transactions of the Linnæan Society: three on Insects; a general arrangement of the Crustacea, Myriapoda, and Arachnides, a very laborious work; two descriptive of ten new genera of Bats; one on three new species of *Glareola*. He died in Italy last year of cholera.

General Joaquim Oliveira,—to whom we are indebted for the present of the *Flora Fluminensis* of Vellozo; a work illustrative of the plants of Rio Janeiro. He held an important office under Don Pedro in Brazil.

Joseph Sabine, Esq., F.R.S., &c.—Mr. Sabine at the time of his death had been a Fellow of this Society for nearly forty years; and as one of its friends who throughout his life devoted himself to the pursuit of natural history there is a claim for justice due to his memory.

He was the intimate associate of many of the oldest and most distinguished of our Members, and there are some around me who unquestionably must have looked on the unkindly feelings cherished towards him of late years with deep regret, and who, without being blind to the errors of judgement he may have committed, still feel that those errors did not implicate his integrity, and that considering his contributions to the stock of our knowledge in horticulture, botany, and zoology, and the kindliness of his nature in promoting the interests of those whom he had it in his power to serve, the obligations of charity were lost sight of in the prejudices by which he was assailed.

But his exertions in the cause of science should not be overlooked nor undervalued; and any one who follows the progression in the development of a more scientific system of horticulture in this country, and an improved taste for the general cultivation of plants, will find that his labours were productive of the best interests in this department of science.

His zoological studies were principally directed to British ornithology, in which he was considered an excellent authority. He had paid much attention to the changes of plumage in birds, to the time

of arrival and departure in the migratory species, and also to the breeding and habits of domestic animals.

He published in the Transactions of the Linnæan Society a paper on a new species of Gull from Greenland; and an account of the Marmots of North America, with a description of three new species; and he wrote the Zoological Appendix to Capt. Franklin's Journey of 1819—1822.

He also contributed two papers on the *Chrysanthemum Indicum* of Linnæus, which he distinguished from what he has named the *C. Sinense*, the common plant of our gardens, imported into Europe in 1789; and there is a paper to appear in the forthcoming Part on the Rose found by Sherard, a genus to which he had paid great attention.

A friend of his has furnished me with a list of forty papers which he contributed to the Transactions of the Horticultural Society, and these may surely be regarded as proofs of the interest he took in its objects and welfare.

I allude to his connexion with that Society with hesitation, because I am ignorant on the subject; but I feel that the claims for justice to the memory of Mr. Sabine will have greater weight, if there be no disposition to conceal the acknowledged evils which arose from his want of method in the management of its finances. Those evils, their causes and effects, I unaffectedly regret, and I rejoice that they have been remedied by the well-directed efforts of others; and with these acknowledgements I hope I may without impropriety quote the charitable sentiments of one who has not been at all times sparing of the literary deficiencies of his cotemporaries. Lord Jeffrey, in his notice of Rogers's poem of Human Life, has this admirable passage, which I think suited to the present occasion:

"When the inordinate hopes of youth, which provoke their own disappointment, have been sobered down by longer experience and more extended views; when the keen contentions and eager rivalries which employed our riper years have expired or been abandoned; when we have seen year after year the objects of our fiercest hostility and of our fondest affections lie down together in the hallowed peace of the grave; when ordinary pleasures and amusements begin to be insipid, and the gay derision which seasoned them to appear flat and importunate; when we reflect how often we have mourned and been comforted, what opposite opinions we have successively maintained and abandoned, to what inconsistent habits we have gradually been formed, and how frequently the objects of our pride have proved the sources of our shame; we are naturally led to recur to the days of our childhood, and to retrace the whole of our career and that of our cotemporaries with feelings of far greater humility and indulgence than those by which it had been accompanied; to think all vain but affection and honour, the simplest and cheapest pleasures the truest and most precious, and generosity of sentiment the only mental superiority which ought either to be wished for or admired."

The Right Hon. Sir John Sinclair, Bart., F.R.S.

Rev. George Henry Storie, M.A., of Camberwell.

Mr. White Watson, of Bakewell.

The names on our Foreign list are those of *Afzelius, Jussieu, Persoon*, and *Schrader*; men to whom I cannot affect to render even the semblance of the respect due to them.

Adam Afzelius, Professor of Botany at Upsal,—was, I believe, the last of the pupils of Linnæus, and distinguished like all the pupils of that great man for his exact botanical knowledge. He contributed two papers to our Transactions: "On the Botanical History of *Trifolium alpestre, medium* and *pratense*," in 1790; and "Observations on the Genus *Pausus*," in 1798.

He resided in Sierra Leone for several years, and published his principal work, "*Genera Plantarum Guineensium*," in 1804; and several Dissertations on the medicinal plants of that country, and some other works.

Antoine Laurent De Jussieu, Professor of Botany, Paris,—one of the original Foreign Members of this Society, author of the "*Genera Plantarum secundum ordines naturales disposita*," and many papers in the *Annales* and *Mémoires du Muséum d'Histoire Naturelle*, in further illustration of his views of the natural system.

The date of the publication of the "*Genera Plantarum*" in 1789, with the fact that the life of this illustrious man terminated at a very advanced age without a second edition of that great work, are proofs of the great acquisitions made in botany within the last forty-five years, and of the hopelessness, save from one individual, of the labours of Jussieu being equalled by any single botanist.

I do not affect to speak of the merits or reputation of this eminent man, but if there were any that can be claimed for him above even the superiority of his intellect and learning, they were those of his modesty and his entire freedom from undervaluing the labours of others; and it is delightful to turn to a letter of his to Sir J. E. Smith and to those of Bernard De Jussieu to Linnæus, to observe how purely these distinguished men regarded their mutual efforts to advance their favourite science.

Christian Henry Persoon, A.M.—The name of Persoon will live as one of the highest classical authorities on the Fungi, for his *Synopsis Plantarum*, published at Paris in 1805, and well characterized by its motto "*In parvo copia*," though highly useful in its day, was naturally doomed to be superseded by later works of a similar kind.

He contributed to our Transactions in 1799 a brief notice of a variety of the Beech found near Göttingen, which he has termed *Quercoides*, from the resemblance of its bark to that of the Oak.

He published between 1796 and 1800 some of his earlier works on Fungi at Leipsic, and his "*Synopsis Methodica Fungorum*" appeared at Gottingen in 1801. This was followed by his "*Icones pictæ rariorum Fungorum*," at Paris, in 1803, and the "*Novæ Species Lichenum*" in 1811.

His collections were purchased by the King of Holland, and the annuity he received for them contributed essentially to the comfort of the later years of his life.

Henry Adolph Schrader, Professor of Botany, at Göttingen,—author of the "*Spicilegium Floræ Germanicæ*" in 1794, and "*Flora Germanica*," vol. 1st, 1806, and various essays on Exotic Plants.

His *Flora Germanica* has a high reputation, but it only extends through the class Triandria. There is a very useful elaborate list of the botanical writers of Germany at the commencement. The *Flora Britannica* of Smith is spoken of in Germany as inferior only to the *Flora Germanica* of Schrader.

At the election which subsequently took place,

His Grace the Duke of Somerset was re-elected President; Edward Forster, Esq., Treasurer; Francis Boott, M.D., Secretary; and Richard Taylor, Esq., Under-Secretary; and the following five gentlemen were elected into the Council, in the room of others going out, agreeably to the by-laws :viz. Walter Buchanan, Esq.; William S. MacLeay, Esq.; the Lord Bishop of Norwich; Richard Owen, Esq.; and Henry F. S. Talbot, Esq.

GEOLOGICAL SOCIETY.

The regular order of our report of the proceedings of this Society having necessarily been interrupted by the Anniversary Proceedings of Feb. 17th, we now return to the papers which had been read previously to that time, in continuation from p. 141.

Nov. 30, 1836.—A paper "On certain elevated Hills of Gravel containing Marine Shells in the vicinity of Dublin," by John Scouler, M.D., Professor of Mineralogy in the Royal Dublin Society, and communicated by Robert Hutton, Esq., F.G.S., was first read.

The object of this communication is to give a brief account of phænomena which, although frequently described in other countries, have been but recently observed in Ireland. Before entering on the immediate subject of the paper, Dr. Scouler gives a general description of the formations constituting the district under examination. They consist of granite, porphyry, quartz rock, micaceous, talcose, and argillaceous schists, greywacké, which near Lyons is succeeded by a very ferruginous conglomerate, and mountain limestone, called, near Dublin, calp.

The principal points at which the author examined the shelly deposits are, the promontory of Howth, Bray Head, and the valley of Glenismaule. On the south side of the promontory of Howth, where the limestone or calp approaches the quartz rock, is a deep depression occupied by an exceedingly tenacious and very ferruginous clay, which also extends across the peninsula, filling up fissures in the limestone. It is unstratified, and does not contain any transported fragments of rocks, but abounds with nodules of oxide of iron, iron pyrites, and oxide of manganese; the last being extracted for œconomical purposes. Resting upon this clay, the limestone and the quartz rock, is a thick accumulation of shelly coarse gravel and fine sand, extending about half a mile in length, but separated into two parts by the hollow in which is situated the village of Howth. The highest portion of the deposit is about eighty feet above the level of the sea. The gravel consists chiefly of limestone, differing in no respect from the limestone of the district; pebbles of argillaceous schist are not uncommon; and rounded fragments of granite, the hard chalk of An-

trim, and flints occur, but rarely; and it is worthy of remark that though the gravel rests partly on quartz rock, fragments derived from it are scarce.

The beds of sand are sometimes very thin, at others of considerable thickness, and though, for limited distances, there appears to be a regular stratification, yet the beds cannot be traced to any extent, thinning out in the same manner as on existing sea-beaches. The shells which have been found were, for the greater part, very imperfect, but Dr. Scouler has been enabled to determine, from well-defined specimens, the following species: *Turritella ungulina*, *Turbo litoreus*, *Nerita litoralis*, *Buccinum undatum*, *Cardium edule*, *Cyprina Islandica*, and *Pecten varians.*

On the opposite side of the Bay of Dublin, and to the south of the promontory of Bray Head, is a similar accumulation extending for upwards of a mile. At its northern extremity it presents a perpendicular section about 200 feet high, but gradually declines towards the south till it sinks to a level with the present shore. It consists, in the upper part, of angular fragments of granite or syenite, and quartz rock; in the middle, of numerous beds of shelly sand and gravel, and in the lowest, of clay and marl. The central beds of gravel are chiefly composed of fragments of limestone of moderate size, and imperfectly rounded, but they also contain pebbles of chalcedony, flint, hard chalk, and a ferruginous conglomerate. With respect to the localities from which these materials were derived, Dr. Scouler says, that no limestone occurs *in situ* nearer than the opposite side of the Dublin granitic mountains; that the fragments of chalcedony, flint, and hard chalk, appear to have been transported from Antrim, and the pebbles of ferruginous conglomerate from Lambay Island, or Lyons Hill, to the west of the Dublin chain. The whole of the recent species of shells found at Howth, have been obtained at Bray Head, with the addition of *Dentalium entalis.*

Besides these shelly deposits which occur adjacent to the existing sea shores, Dr. Scouler describes others at a considerable distance inland. One of the most remarkable of these, is in the valley of Glenismaule, near the source of one of the branches of the Dodder, and about seven miles from the Bay of Dublin. On each side of the valley are perpendicular cliffs formed of irregular beds of sand and calcareous gravel, about 100 feet thick, and probably 200 feet above the level of the sea. These beds are also above the level of any of the calcareous strata of the immediate neighbourhood. Associated with the limestone fragments are pebbles of flint and chalcedony, as well as recent shells identical with those in the beds previously described at Howth and Bray Head. Dr. Scouler also mentions having found a specimen of limestone perforated by *Lymnoria terebrans.* Similar deposits are stated to exist in other valleys in the vicinity of Dublin; and accumulations of gravel, agreeing in the arrangement of the beds but differing locally in the nature of the materials, to extend over the whole of Ireland, forming low rounded hills, and filling previously existing depressions.

The only instance of remains of mammalia in this gravel, known to

Dr. Scouler, is the discovery of bones of the Irish Elk, at Enniskerry, near Dublin.

The following inferences were then given, as deducible from the facts contained in the memoir:

1st, That the coast around the Bay of Dublin has been elevated, though unequally, at a comparatively recent geological epoch; 2ndly, That the valley of Glenismaule, and other valleys containing similar accumulations of drift, were at one time under water, and then filled with calcareous gravel; and that they were afterwards elevated, and subsequently re-excavated by the action of running water.

The memoir concluded with some theoretical observations respecting the sources from which the calcareous gravel was derived, and the agents by which it was brought into its present position.

A paper "On the Geology of the Thracian Bosphorus," by Hugh Edwin Strickland, Esq., F.G.S., and William John Hamilton, Sec. G.S., was then read*.

The formations which occur in the neighbourhood of the Bosphorus may be classed as follows: 1. A series of beds considered to be equivalents of part of the Silurian system; 2. Igneous rocks; 3. Tertiary limestone; 4. Ancient alluvium.

1. *The equivalents of the Silurian system* occupy both sides of the Bosphorus for about three quarters of its length, and extend in Europe and Asia towards the W.N.W. and E.S.E. to an unascertained distance. The prevailing rock is argillaceous schist, but associated with it are compact brown sandstone, and compact dark blue limestone, the whole passing insensibly into each other. Andreossy and an American traveller referred the deposits to the transition class, on mineralogical characters; and the authors of this memoir, to that portion of it lately named Silurian by Mr. Murchison, from the general agreement of the organic remains to those found in the formations beneath the old red sandstone in England. Fossils are, however, of so rare occurrence that Messrs. Strickland and Hamilton noticed them at only two localities, a ravine above Arnaout-keui, about four miles from Pera on the European side; and the Giant's Mountain on the Asiatic side of the Bosphorus, and about fifteen miles from Constantinople. At the former locality they were found in argillaceous schist, and in the latter in limestone. They belonged chiefly to the genera *Spirifer*, *Productus*, *Terebratula*, *Atrypa* and *Orthis*; but the eye of an *Asaphus*, remains of *Crinoidea*, and of three genera of *Corals* were also obtained.

2. *Igneous Rocks.*—The transition rocks are united on the north to a mass of igneous rocks, and on the south-west to tertiary deposits. The authors were unable to determine the relative age of these two formations; but as the igneous rocks are brought into more immediate connexion with the Silurian or transition group than the tertiary, they are described first. They consist of trachytes and trachytic con-

* In a note, Mr. Strickland says, that Mr. Hamilton being still in Asia Minor, he has been deprived of any direct assistance in drawing up this paper; and that he is solely responsible for any theoretical views which it may contain.

glomerates. The former are more or less compact, sometimes passing into phonolite and basalt, and occasionally assume a columnar structure. The conglomerates are composed of angular fragments of trachyte, imbedded in a tufaceous paste. The inclosed portions are sometimes softer than the cement, when the rock assumes a honeycomb appearance, but they are more often harder, and stand out in bold relief. The conglomerates rest upon and alternate with the trachyte, and in some places are intersected by basaltic dykes. Veins of carnelian and chalcedony are stated to be contained in the igneous rocks, and near Filbornou to pass through the conglomerate, traversing both the basis and the included fragments. These conglomerates are considered by Mr. Strickland to have been accumulated by water, and the contained fragments, though commonly angular, are sometimes rounded, and included in finely laminated volcanic sand. On the Asiatic side of the Bosphorus the igneous rocks commence *en masse* at Kavak, under the old Genoese Castle, and extend to Yoom-bornou on the Black Sea, or perhaps further; and on the European side they commence on the north of Buyukderé, and also extend to the Black Sea. Besides these great masses of igneous products, trachytic and trap dykes were observed by the authors traversing the Silurian rocks at Baltalimani, in the hills above Bebek, at Kiretch-bornou, and the base of the Giant's Mountain.

3. The *Tertiary deposit* commences immediately on the west of Constantinople, and extends inland about three miles, till it meets the transition formations, and ranges along the north coast of the Sea of Marmora for many miles, its western limit being at present undefined. It is best exhibited in the quarries at Baloukli and Makri-kuei, where it consists of horizontal beds of soft, white, shelly limestones and marls, resting on sand in which no fossils have been observed. Near Constantinople the deposit was apparently accumulated in an æstuary, for it contains a species of Cardium, and considerable numbers of a fossil considered to be a Cytherea, the whole being associated with land and freshwater shells, some of which resemble recent species.

Along the banks of the Bosphorus the authors observed no traces of a tertiary formation; and consequently infer that this channel was opened at a comparatively very recent period.

The only ancient alluvium mentioned in the memoir is an extensive and thick deposit of ferruginous clay, sand, gravel and boulders, resting upon the Silurian or transition rocks. It commences a few miles north of Constantinople, forms the subsoil of the Forest of Belgrade, and apparently skirts the southern side of the Lesser Balcan range.

Dec. 14.—A paper "On Impressions in Sandstone resembling those of horses' hoofs," by Charles Babbage, Esq., and communicated by the President, was first read.

During a recent visit to Dowlais, Mr. Guest mentioned to the author, that in the channel of a stream on the extensive moor called Pwll y Duon, and about seven miles from Merthyr Tydvil, there were many impressions considered by the country people to have been made by horses' hoofs. The stratum of sandstone in which they occur is called the Farewell Rock, being the lowest bed of the

coal measures. At first sight they presented a strong resemblance to the marks which the hoof of a horse would leave on a soft surface, but on a closer examination Mr. Babbage found that the part which should have received an indentation from the frog was in relief, and resembled rather a cast of the frog itself. The first mark examined by him proved to be the letter G, which had been carved on the rock by some person whose initials were G. H. This discovery made him inspect the others more minutely, and he ascertained satisfactorily that they were not artificial. Similar impressions were noticed by him at several places on the moor.

The author then referred to analogous casts in the old red sandstone of Forfarshire, and there called Kelpies' feet.

In attempting to account for the marks, Mr. Babbage described some observations recently made by Mr. Lyell on impressions left by Medusæ on the rippled sand near Dundee. On removing the gelatinous body of the animal, a circular space was exposed, not rippled, but having around half the border a depression of a horse-shoe form. These marks, however, were not considered by Mr. Lyell as identical with those called Kelpies' feet, but merely so far analogous as to invite further observations, and to make it desirable to possess drawings of the impressions which different species of Medusæ leave, when thrown by the tide upon a beach of soft mud or sand.

A memoir "On the occurrence of silicified trunks of large trees in the new red sandstone formation or Poikilitic series, at Allesley, near Coventry," by the Rev. Wm. Buckland, D.D., Professor of Geology and Mineralogy in the University of Oxford, was then read.

In the great bed of gravel which overspreads the portion of Warwickshire referred to in this paper, specimens of silicified wood had been long found, and from being slightly rolled, it was conjectured that they could not have been drifted from a distance, though no indication of their original matrix had been observed. In the spring of last year, however, Dr. Buckland was informed by the Rev. W. T. Bree of Allesley, that part of the silicified trunk of a tree, several feet in length and a foot and a half in diameter, had been discovered in the garden of the Rev. Mr. Gibson, at the bottom of Allesley Hill. On visiting the spot in October last, the author ascertained that the tree was not imbedded in the gravel, but in that portion of the new red sandstone series, which consists of indurated sandstone, alternating with beds of conglomerate, chiefly made up of sand and of pebbles of quartzite and compact forms of trap.

In the churchyard of Allesley Dr. Buckland found an angular fragment of similar silicified wood which had been fresh cast up from the bottom of a grave, sunk to an unusual depth in the red sandstone; and in making a road from Allesley towards Coventry another large tree was discovered a short time ago, and the greater part of it used in forming the foundation of the road. On comparing the fragments found in the gravel with the tree in Mr. Gibson's garden, which is carefully preserved in its matrix, Dr. Buckland found so perfect an identity in mineral character as to leave no doubt that the fragments in the surface gravel had been derived from denuded beds of the new red sandstone.

A description was then given of the mineralized condition of the wood, and its organic structure. On the surface of many of the specimens from the gravel, is a multitude of small spherical cavities, each of which was once filled with a minute round concretion of oxide of iron or imperfect jasper; and innumerable specks formed by these concretions pervading the interior of the specimens, appear to have been formed in a manner analogous to that which produced the eye agates in the Antigua wood. The tree in Mr. Gibson's garden, and many of the larger fragments found in the gravel, abound with minute longitudinal apertures resembling those in shrunk and shaken timber; many of these are filled with red oxide of iron, or lined with beautiful crystals of dark-coloured quartz. In two specimens Dr. Buckland noticed longitudinal holes about $\frac{1}{4}$th of an inch in diameter, which had apparently been perforated by the larvæ of some insect. In the large collection formed by Mr. Bree, the author sought in vain for examples of the petrified palms, psarolites and helmintolites described by Cotta and Sprengel as found in Saxony, in beds considered to be the equivalents of the new red sandstone of England; all the Allesley specimens are either referrible to decided coniferæ, which have distinct concentric lines of growth, or exhibit a compact structure in which neither large vascular tubes nor concentric lines of growth are visible.

A paper entitled "Further notice on a partially petrified piece of wood from an ancient Roman aqueduct at Eilsen, in the Principality of Lippe-Bückeberg," by Charles Stokes, Esq., F.G.S., was next read.

Since his former communication on this subject, (Lond. and Edinb. Phil. Mag. vol. ix. p. 499) the author has been shown by Mr. Robert Brown a specimen from the same partially petrified piece of wood; and Mr. Brown has pointed out to him, in its longitudinal section, that the petrified portions are spindle-shaped bodies, about two inches in length, in some instances completely inclosed within and surrounded by the unchanged wood, and are not, therefore, as formerly conjectured by the author, connected by such an external supply of carbonate of lime to particular points as might have been derived from stalactites formed in the building.

The author also stated that Mr. Brown had called his attention to the remarkable circumstance exhibited in this specimen, that though the change in the longitudinal fibres appears to be complete, yet the medullary rays are still in their ligneous state; and on referring to the specimen formerly described, Mr. Stokes has found some instances in which a part of the medullary ray which passes through the petrified portion has not been so completely changed as the surrounding longitudinal fibres, or the part of the same ray which is more in the centre of the petrified portion.

Of the unpetrified part of the specimen, some portions are much decayed and worm-eaten, while others are hard and apparently in good preservation; the line of separation between the two conditions being occasionally remarkably well defined. On submitting portions of each to the action of muriatic acid, Mr. Stokes found that the decayed part exhibited only a slight effervescence, while that which appeared in good preservation gave off a much greater quantity of gas, and

chiefly from the inside of the larger vessels, as if they were coated with an extremely thin layer of carbonate of lime. This fact, connected with the medullary rays remaining in some instances unchanged, or but partially changed, presents, as stated by the author, the first ocular demonstration of progressive steps in the process of petrifaction. The communication concluded with some observations on the fossil wood of Allesley described in Dr. Buckland's paper, in some of the specimens of which there are spindle-shaped portions similar to those in the partially petrified wood of Buckeberg.

"Description of a Raised Beach in Barnstaple Bay, on the north-west coast of Devonshire," by the Rev. Professor Sedgwick, F.G.S., and Roderick Impey Murchison, Esq., F.G.S., was afterwards read.

This raised beach is first seen at the northern extremity of the blown sand-hills called Braunton Burrows, and thence may be traced round the western end of Saunton Down, into Croyde Bay. After meeting with some interruptions it reappears, and may be followed to the face of the bold headland called Baggy Point, about three miles from the place of its commencement. In situations where it is best exposed, especially on the south side of Saunton Down, it puts on the form of a horizontal under terrace, resting upon an indented and irregular surface of the older formations, and abutting against their component beds. It forms regular sea-cliffs, the stratification of which is most distinct; and the several beds may be traced by small bands of shingles, by alternating courses of different degrees of fineness, and by horizontal lines of division. In distinctness of stratification it yields to no rock; and as several parts of the cliff are in a state of perfect induration, presenting regularly bedded masses of calcareous grit and sandstone, the authors at first sight mistook it for a secondary formation.

The bottom of this horizontal deposit is chiefly composed of indurated shingles resting on the ledges of the older rocks, and filling up their inequalities. These conglomerates or shingles are seldom of great thickness, but in some places alternate two or three times with beds of sand, so as to reach an elevation of eight or nine feet in the horizontal deposit. Over the shingles are horizontal beds of sand, occasionally indurated, sometimes putting on a concretionary structure, and weathering into grotesque forms by the action of the elements. Lastly, the preceding deposits are surmounted by regular beds of finely laminated sand in a state of imperfect induration, and sometimes hardly differing from the sand of the actual beach between the high and low water marks. The thickness of these beds of sand amounts in some places to more than twenty feet. These marine deposits are frequently covered by terrestrial overshot materials which have descended from the higher talus of Saunton Downs. In the whole of the stratified under cliff above described there are sea shells. In the upper part they are rare, and in a bad state of peservation; but in the lower and more indurated portions they are more abundant, are often well preserved, sometimes appearing in beds, and in their condition and arrangement exactly resembling the shells of a modern beach. In species, they are identical with the living shells of the coast.

Among them the authors enumerate *Mactra stultorum, Tellina fabula, T. solidula, Cardium edule, Ostrea edulis, Mytilus edulis, Mya margaritacea, Pholas, Patella vulgaris, Natica canrena, Purpura lapillus*, &c., making in all twelve or fourteen species*.

At the north side of Croyde Bay the sea shells are very abundant in the deposit; the lower shingles expand to the thickness of nineteen feet, and are found on the face of Baggy Point at various heights and rising to sixty or seventy feet above high water level.

The horizontal beds, above described, cannot have been formed by accumulations of blown sand. They are stratified marine deposits, differing in no respect from the sand and coarsest shingles of the neighbouring beach, except in the level; and they perfectly demonstrate an elevation of the neighbouring coast during the *modern period.*

In confirmation of their views, the authors assert that the physical features of the neighbouring region, indicate that kind of depression in the sea level which is demonstrated by the raised beach. The ancient line of sea-cliffs may be traced inland by Saunton and Braunton towards the mouth of the Barnstaple river, passing to the east of the existing marshes and dunes of blown sand. In like manner the old line of cliffs, anterior to the elevation, may be traced from Appledore along the south side of Norton Burrows. The popple or pebble beach, a remarkable ridge of large rounded blocks of stone, rising to a height of seventeen feet above the sea level, and shutting out the ocean from the neighbouring marsh lands, &c., of Appledore, is, in connexion with the blown sands, regarded by the authors as the natural and necessary consequence of a considerable elevation of the coast, and as strongly confirming the views they have advanced. In further support of these views, they state that the raised beach of Barnstaple Bay, forms only one of a connected series of phænomena, all tending to demonstrate the same conclusion, viz. the occasional changes of high-water level, both in the way of elevation and depression, within a comparatively recent period. They point out the conditions under which raised beaches (admitting their hypothesis) may or may not be expected; and they affirm that there is a connected series of phenomena both on the north and south coasts of Devonshire and Cornwall, in perfect accordance with what they have described. The raised beach of Hope's Nose is the most striking instance in South Devon; and they bear witness to the correctness of Mr. Austen's description of it, and to the justice of his conclusions. On the coasts of Cornwall phænomena of like kind are very numerous. Not only is there incontrovertible proof of raised beaches at various levels, but in some places long smooth waterworn surfaces, exactly like those formed by the existing breakers of a rocky shore, may

* This list has been much augmented by the subsequent labours of Major W. Harding, F.G.S. of Ilfracomb, who has in other respects confirmed the views of the authors, and added some important facts. In the craggy cliffs of the old transition rocks near Baggy Point, he has detected patches of the indurated shingle, or "dry beach" as he terms it, containing modern sea shells at *different heights*, far above the reach of the highest tides.

be traced midway in the cliff, at an elevation quite out of the reach of the cause which formed them.

Lastly, the authors enter on some details as to the quantity of elevation, proved by the phænomena of recent marine deposits in different parts of England. They state that the raised beaches of South Devon and Cornwall indicate various changes of level, from ten to forty feet, the phænomena of depression, of which there are examples, not being considered. That the greatest elevation in North Devon seems to have been about 70 feet, while in Lancashire, Cheshire, and Shropshire, there are marine deposits, containing also shells of existing species, at various elevations of from 300 to 500 feet. The intensity of elevatory force seems therefore to have increased towards the north, and perhaps reached its maximum among the Cumbrian mountains, from which enormous masses of materials have drifted in various well-known directions.

The country of Siluria and South Wales, the detritus of which was described by Mr. Murchison on a former occasion, is of course specially exempted from the application of this inference; since that region has been shown to have been elevated at an antecedent period*.

On the same evening, after the ordinary business of the Society had terminated, a Special General Meeting being held for the purpose of electing two Secretaries in the place of William John Hamilton, Esq., and Woodbine Parish, jun. Esq., who had retired from the office, the scrutineers reported that Robert Hutton, Esq., and John Forbes Royle, M.D. F.L.S., were duly elected.

ZOOLOGICAL SOCIETY.

(Continued from p. 306.)

November 8, 1836.—A letter, addressed to the Secretary, by Robert Mackay, Esq., the British Vice-Consul at Maracaibo, and a Corresponding Member of the Society, was read, describing the habits of a *Vulture* (*Vultur Papa*, Linn.) forwarded to the Society for the Menagerie, but which had unfortunately died during the voyage.

After noticing the peculiar habit attributed to these birds, (which frequently congregate to the number of three hundred,) of paying deference to an individual differing from the rest in plumage, and to which the inhabitants of Maracaibo give the title of king, Mr. Mackay proceeds to state:

"These birds, in their flights, ascend to such a height as to be lost sight of, and from their elevation, discover objects of prey.

"They reside in the savannas of a warm and dry temperature; and their travels do not extend beyond five or six leagues of the place where they have been bred.

"They lay their eggs, and hatch their young, in the small concavities of mountains.

"At a distance from towns, villages, and frequented roads, they

* Geol. Proceedings, Vol. II. p. 77; or Lond. and Edinb. Phil. Mag. vol. viii. p. 566.

generally assemble in large numbers; but in the immediate vicinity of such situations the king never deigns to associate with his vassals."

At the request of the Chairman, Mr. W. Martin read the following description of a new species of the genus *Felis*.

"The beautiful species of *Felis* to which I beg leave to call the attention of the Meeting was brought from Java or Sumatra, and obtained, with other specimens from the same locality, from Mr. Gould. The only writer, as far as I can learn, who notices it, is Sir W. Jardine in the 'Naturalist's Library,' in which work are two figures from specimens in the Edinburgh Museum; but he there confounds it with the *Felis Diardi* of Cuvier, to which species, as indeed also to the *Felis Bengalensis*, it bears a close affinity in the style and colour of its markings. It will be easy, however, to show that the *Felis Diardi* is a very different species to the present. The first description of the *F. Diardi* is in the fourth volume of Cuvier's *Ossemens Fossiles*, p. 437. 'There is,' says Cuvier, 'in Java another *wild Cat* larger than *Felis Bengalensis*, very remarkable for the beautiful regularity of its blotches, of which Messrs. Diard and Duvaucel have transmitted to us a skin and a drawing. We shall designate it *Felis Diardi*.' After describing its colour, he adds, 'The head is six inches, the tail 2 feet 4 inches, the body 2 feet and a half, and its height at the shoulder must be 18 inches.' (French measures.) With regard to the *Felis Diardi*, it is somewhat questionable whether it be distinct from the *Felis macrocelis*, or not; at all events it is a large *Cat* closely allied to, if not identical with that animal, but certainly distinct from the *Cat* before the Meeting.

"The admeasurements of this species are as follows:

	Feet.	Inches.
Head and body	1	11
Head from nose to occiput, following the arch of the skull	0	$5\frac{1}{2}$
Tail	1	$3\frac{1}{2}$
Height at shoulder	0	$10\frac{1}{2}$
Total length	3	$2\frac{1}{2}$

"It may be observed, that the individual is adult, as proved by the state of the dentition; its colouring agrees closely with that detailed by Sir W. Jardine. The ground tint is rusty grey the rufous tinge prevailing on the top of the head down the middle of the back, over the cheeks, chest, scapulæ, fore limbs, and thighs. On the top of the head are two longitudinal markings of black inclosing a space cut up by irregular small rings or dashes of black, and external to these begin two decided black lines (commencing over each eye), which become broader on the occiput and back of the neck, on which latter part they converge, but do not come in contact with each other; they then sweep over the top of each shoulder, blending with the markings of the body.

"Continued from the first-described central markings on the head, there runs between these two decided stripes a broken line, assuming between the shoulders the form of elongated open spots, and

ultimately a black dorsal stripe continued to the base of the tail; on the haunches, however, it divides into two parallel stripes. The ears are short and somewhat rounded, black at the tips, grey in the centre, and black at and around their base; beyond the black mark at their base, there is a space of dusky grey, which merges into the colour of the neck. The sides of the neck, scapulæ, fore and hind limbs, are thickly spotted with black. The sides of the body are marbled with obliquely longitudinal marks of dark grey, each mark having an irregular margin of black.

"The lower angle of each eye is black, and two black lines cross the cheek, passing into a throat-mark carried across beneath the angle of the lower jaw; below this is a similar mark but more indefinite; the chest is spotted with black. The abdomen is dirty white which is crossed by rows of black spots in regular order. The upper surface of the tail is grey, the lower yellowish grey; it is marbled by spots of black forming indistinct rings, which, towards the tip, assume a more definite character; the extremity being black. The fur of the body is moderate and sleek; on the tail it is full and soft.

"For this beautiful species of *Cat* I venture to propose the title of *Felis marmorata*. Though inferior in size to the *Felis macrocelis*, this species is related to it, not only in the style of the markings of the fur, but in the elongation of its form, and the length and thickness of the tail; it is a *Rimau Dayan* in miniature; nor, though larger than the *Felis Bengalensis*, is it less allied to that species, between which and the former it constitutes an intermediate grade."

November 22, 1836.—A communication from Mr. Harvey, of Teignmouth, in Devonshire, was read, which referred to a specimen of the *electric Ray* then on the table. The fish was caught in a trawl-net near Teignmouth, and was presented to the Society by Mr. Harvey. When taken, part of a specimen of the small spotted *Dog-fish* was hanging from its mouth. The fishermen handle the *electric Ray* while it is alive without being at all affected by it, always taking care to lay hold of the tail.

Mr. Yarrell exhibited a very large *Carp* taken by a net in a piece of water called the Mere, neare Payne's Hill, in Surrey. The length of the specimen was 30 inches, the girth of the body at the commencement of the dorsal fin 24 inches; the weight, 22 pounds. The fish belonged to Edward Jesse, Esq., author of the "Gleanings in Natural History," by whose permission it was exhibited. Mr. Yarrell observed, tbat he could find no record of any *Carp* so large having before been taken in this country.

Mr. Martin, at the request of the Chairman, read the following notes on the anatomy of *Koala, Phascolarctos fuscus*, Desm.

"The acquisition of a young male *Koala* preserved in spirits, and presented to the Society by Captain Mallard, has afforded me the opportunity of examining the *viscera* of this rare and curious animal; which I did with the utmost care. Differing from the *Wombat* in its *dental formula*, in which respect it closely resembles the *Kangaroos*, the visceral anatomy of the *Koala* closely approximates to that of the former animal, as will be perceived by comparing the follow-

ing notes with the description of the anatomy of the *Wombat* by Mr. Owen.

"On reflecting the skin of the *abdomen*, there appeared a small transverse muscle arising from the skin on either side, which passed over the marsupial bones, towards their upper extremity, acting as a support to, and a compressor of them.

"The *pyramidalis* muscle, to which, on its outer side is attached the inner edge of the marsupial bone, radiated from this bone to the middle line, and sent off a broad *fascia* of fibres over the *rectus muscle* to the cartilages of the ribs. The *rectus* began broad from the cartilages of the lower ribs, its fibres appearing to mix with those of the *pectoralis*; it continued its course broad to the *pubis*, and was inserted in the usual manner. The *external oblique* was thick and its fibres remarkably strong; the *internal oblique* gave off a strong *cremaster*, which ran down the spermatic cord as far as the *testis*.

"The *transversalis* as usual.

"The first head of the *triceps adductor femoris* was connected by a slip of fibres to the external apex of the triangular base of the marsupial bone, giving to that bone, by its contraction, a slight external motion.

"The *panniculus carnosus* was very strong, especially over the back and sides.

"The capacity of the *thorax* was very small in comparison with that of the *abdomen*.

"The stomach occupied the left side of the abdominal cavity, scarcely passing the mesial line; its pyloric portion bent down abruptly, forming a narrow arch through which protruded the *lobulus Spigelii* of the liver.

"The liver consisted of two equal parts, a right and left, both closely attached by membranous (or peritoneal) processes to the diaphragm; the *ligamentum latum* verged towards the left side. The right portion of the liver was divided into three foliaceous lobes, the left into two: the free edges of this *viscus* were deeply and abruptly fissured, as if cut with a knife; and its under surface presented an irregular congeries of small *lobuli* or appendages, clustered thickly together; on the left side, the outer lobe of the liver passed completely behind or dorsad of the stomach, the cardiac portion of which advanced as low as the left kidney. The outer lobe of the liver on the right side advanced in a pointed form, and passed behind the whole of the dorsal surface of the right kidney. The great mass of the liver had, in fact, a dorsad position, the anterior portion being comparatively very trifling.

"The gall-bladder was seated in the fissure between the first and second lobes, reckoning from the right side; it was very large, but empty. Of great width at its base, it narrowed gradually to an almost vermiform apex, and its total length was $3\frac{1}{4}$ inches. Its duct, of considerable calibre, terminated exactly one inch below the *pylorus*.

"The spleen was long, thin, and tongue-shaped; it lay loosely adhering to the *cardium*; its greatest breadth was $\frac{1}{2}$ an inch, its

length, $2\frac{1}{4}$ inches; its edges were very thin and slightly crenulated.

"The pancreas presented a thin, flat portion, attached to the spleen, whence ran a broad slip attached to the peritoneal reflection at the back of the stomach, and advancing round to the *duodenum.* Its duct joined that of the gall-bladder $\frac{3}{4}$ of an inch from its insertion.

"The stomach was divided by a contraction, into two distinct portions; of these, the cardiac was large and almost globular, its breadth across being 2, its length across $2\frac{1}{2}$ inches; its *parietes* were much thinner than those of the pyloric portion, which, as we stated, bent down abruptly, so as to form a narrow arch. The breadth of the *pylorus* at its commencement, was little more than an inch, but it swelled out into a *sacculus,* whence it narrowed to the pyloric orifice. Following its greater curve it measured $2\frac{1}{2}$ inches, along its smaller, only $\frac{3}{4}$ of an inch. It was slightly puckered transversely on the sides by a posterior longitudinal band of fibres. Anterior to the entrance of the *œsophagus,* and occupying the space of the smaller curvature of the stomach, between the *œsophagus* and the contraction, was situated a large thick gland, opening by numerous ducts, whose mouths clustered together, formed a sort of network. On each side of this gland the inner membrane of the stomach was longitudinally corrugated with small *rugæ,* whence larger *plicæ,* and more distinct from each other, were continued down the inner surface of the *pylorus,* to its orifice, which was closed with a strong sphincter-valve; the cardiac pouch was lined with a thin smooth cuticular membrane. The *duodenum* began pyriform with a small *sacculus* $\frac{3}{4}$ of an inch in breadth, whence it narrowed to $\frac{5}{8}$ of an inch; this being its average breadth. Its course was as follows: Leaving the *pylorus,* and bound to the spine by mesentery, it advanced over the right kidney, then crossed the spine, turned up on the left side under the cardiac portion of the stomach, and merged into *jejunum.* The whole of the inner membrane of the small intestines exhibited a beautiful velvety tissue.

"The *cæcum* was of enormous magnitude, and slightly puckered equidistantly or nearly so throughout its whole length into *sacculi,* by a slight longitudinal (mesenteric) band of muscular fibres; there appeared also faint traces of an opposite band. Turning spirally on itself and beginning large, it gradually narrowed, the decrease of its last portion, for the length of 18 inches, being very marked; this portion running to a long vermiform point. The total length of the *cæcum* was 4 feet 2 inches. Basal breadth, 2 inches. The *colon,* resembling in character the first portion of the *cæcum,* was slightly contracted into large *sacculi,* the first *sacculus* just below the entrance of the *ileum,* being more decided and larger than those which succeed; it was, however, nothing more than a simple enlargement, without any pyramid figure. After a course of 17 inches, the *colon* decreased in size to the breadth of $\frac{5}{8}$ of an inch; the total length of the large intestines was 6 feet 4 inches. The inner membrane of the *rectum* was corrugated longitudinally.

"The lungs consisted of 3 right lobes, one large, and two small; and of two left lobes, the lower by far the largest.

"The heart was compressed and pointed; its length was two inches.

"The *aorta* gave off as usual 3 branches for the supply of the anterior portion of the body. The first or *arteria innominata*, however, almost immediately divided into carotid and subclavian. The right auricle presented at its upper part a semilunar notch fitting to the base of the *aorta*, two points rising up, one on each side of the *aorta*, as auricular appendages. Into the upper part of the auricle just behind the right appendix entered the right *vena cava superior*; and into the inferior portion of the auricle close to the entrance of the *vena cava inferior*, entered the left *vena cava superior*. The *vena azygos* running up on the left side of the *aorta*, entered the left *vena cava superior* an inch from its termination. This arrangement of the *venæ cavæ* appears to be normal in the *Marsupials*, as Mr. Owen has previously observed.

"Six coronary veins entered the right auricle round its junctional margin with the ventricle.

"The auriculo-ventricular opening on the right was of moderate size, with a simple valve, the edges of which were bound down by the tendons of two distinct *carneæ columnæ*; a third *fasciculus* of fleshy fibres, but very indistinct, were to the right of these, but they could hardly be said to constitute a third *carnea columna*. The right ventricle does not approach the *apex* of the heart by $\frac{3}{8}$ of an inch. No trace of *foramen ovale*. Pulmonary artery very wide, dividing after a course of $\frac{1}{2}$ an inch in two branches, a right and left. Right ventricle very thin; the left, very thick and firm.

"Of the kidneys, the right was seated higher, nearly by its whole length, than the left; the lower end of the former and the upper end of the latter being parallel. In shape, these organs were oval, and but slightly compressed. Their *pelvis* was small, the *papilla* single and obtuse; the cortical and cineritious layers very distinct. Length, $1\frac{3}{8}$ of an inch; breadth, $\frac{3}{4}$ of an inch.

"The *penis*, of small size and conical figure, was placed immediately anterior to the *anus*; it was slightly bifurcate, or rather had two projecting *papillæ*, one on each side of the urethral orifice. Length of spongy portion, $\frac{3}{4}$ of an inch. Bladder small, oval, and much contracted. *Testis*, of the size of a horsebean. Total length of *vasa deferentia*, $2\frac{1}{2}$ inches; their entrance was below and external to the ureters, which opened as usual. Prostate small. *Vesiculæ seminales* small; they entered $\frac{7}{8}$ of an inch below the bladder, with Cowper's glands, which were as large as a tare.

"The thyroid glands were oval, compressed, and small; their colour pale; they began at the 4th ring of the *trachea* from the thyroid cartilage, and extended to the 9th or 10th.

"There was a round subzygomatic gland the size of a pea on the *masseter*, and two others of the same character were placed on the front of the neck, on the *platysma myoides*.

"The submaxillary glands were thin and long, measuring 1 inch in length. Their situation was as usual.

"The parotid glands, very extensive but superficial, occupied the

usual situation; the duct passed over the *masseter*, and entered opposite the 3rd molar, anterior to the edge of the *buccinator*.

"The *sterno-cleido-mastoideus* was attached not only to the mastoid process, but also to the whole extent of the occipital ridge; it consisted of two portions arising as usual, from clavicle and sternum.

"The tongue was thick at its base, which rose abruptly from a deep furrow surrounding its root; the distance from its root to the *epiglottis* $\frac{1}{8}$ of an inch. Its form was narrow, equal, and rounded at the tip; its surface was velvety, and one large central *papilla* was seated near its base. Length altogether 2 inches. Breadth $\frac{1}{2}$ an inch. Length of free part $\frac{3}{4}$ of an inch. The palate was divided by elevated transverse ridges into 8 furrows.

"*Pharynx* spacious, and lined with a corrugated membrane. *Œsophagus* narrow, its inner membrane being puckered longitudinally.

"The anterior surface of the thyroid cartilage was regularly convex, but not so protuberant as in the phalangers; nor did the *os hyoides* play freely over it."

Mr. Edward Burton, of Fort Pitt, Chatham, communicated a description of a small species of *Pipra* received from the Himalaya mountains, and considered by Mr. Burton to be the first species of this genus yet discovered in those regions. He characterized it as *Pipra squalida*.

PROCEEDINGS OF THE FRIDAY EVENING MEETINGS OF THE ROYAL INSTITUTION.

(Continued from p. 318.)

April 7th.—Mr. Dent on the construction and manufacture of clocks and watches.

April 14th.—Mr. Griffiths on capillary and cohesive attraction, and their application to the art of veneering.

April 21st.—Mr. S. Solly on the central portions of the nervous system in the animal kingdom.

April 28th.—Mr. Faraday on a peculiar condition of iron in relation to its chemical affinity and its electromotive force. (See the papers by MM. Schœnbein and Faraday in our preceding and present volume, including two by the former in the present Number.)

May 5th.—Dr. Boase on tin and its application to the arts.

May 12th.—Dr. Mantell on the Iguanodon and other fossil remains discovered by him in Tilgate Forest.

May 19th.—Mr. Warren De la Rue on the history and manufacture of playing-cards.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

April 17.—A meeting of the Philosophical Society was held on Monday evening, the Rev. Dr. Clark, the President, being in the chair. The Rev. L. Jenyns made some remarks on the unusual degree of cold which prevailed during March. It was stated that the mean temperature of the month, as deduced from observations made

at Swaffham Bulbeck, was only 36°·2, being the same as that of January, and more than six degrees lower than the *average* mean for March. The maximum was only 49°, and the minimum 11°; this last, which was a lower temperature than any experienced since the hard winter of 1829–30, having occurred on the morning of the 24th.

Professor Willis exhibited and explained a machine which he terms a Tabuloscriptive Engine. The object of this machine is to transfer to paper any numerical series of magnitudes, so as to exhibit the curve which would be obtained by making those magnitudes a series of ordinates; a process of very frequent and important use in comparing the results of observations of various kinds, as, for instance, meteorological, tidal, and statistical observations. The machine takes three places of figures, is capable of being worked with very slight attention, and with great rapidity, and produces a sheet very readily legible and intelligible.

May 1.—A meeting of the Philosophical Society was held on Monday evening, Dr. Thackeray, V.P., in the chair. A paper by A. Moore, Esq., of Trinity College, was read, on the solution of a difficulty of analysis noticed by Sir W. R. Hamilton.—Mr. Whewell gave an account of the performance of a new Anemometer, invented by him, which has been erected at the top of the house of the Society, and also on the top of the Observatory, and of which the indications for the last four months have been recorded.—Mr. Kelland also read a paper on the effect of the elasticity of the æther in crystals as bearing on the undulatory theory.

May 15.—A meeting of the Philosophical Society was held on Monday evening, Dr. Clark, the President, being in the chair. A communication by Mr. G. Green was read, containing the solution of a problem respecting the motion of a heavy fluid in a canal.—Mr. Hopkins gave an account of his mathematical researches on the refrigeration and the internal fluidity of the earth.—A paper of Mr. Moseley was read, on the theory of the equilibrium of bodies in contact.—Mr. W. W. Fisher presented another communication on the subject of *spina bifida.* The author has observed, in two cases of *spina bifida*, irregularities which he described; i. e. the union of two or more of the sacral ganglia; the passage of their respective nerves through the sheath in one fasciculus, and the adhesion of the extremity of the spinal marrow to the walls of the sac. By applying to the consideration of these anomalies the knowledge which we now possess of the formation of the different portions of the nervous system in the embryon, and of the anatomy of that system in the lower order of animals, he is led, with regard to the two examples he has seen, to adopt the following views.

1. That the union of the sacral ganglia constituted the primary irregularity to which the anomalous distribution of their corresponding nerves between the ganglia and the spinal cord might be referred.

2. That, unacquainted with any circumstance connected with the formation of the spinal cord and its sheath, which can account for

the adhesions between them, he is induced to attribute these adhesions, by which the ascent of the extremity of the cord to its usual position was prevented, to the irregular manner in which the nerves had become inserted in the spinal marrow.

3. That the union of the ganglia may be in some measure ascribed to the development of a process by which the adjoining ganglia are in some instances, even of normal conformation, connected with each other, and that the general occurrence of the deformity at the lower extremity of the spinal column may be attributed to the relative position of the sacral ganglia, which are placed within the sacral canal, those of the other spinal nerves being placed in the intervertebral foramina.

4. That the incomplete formation of the posterior wall of the spinal column is rather to be attributed to the interference occasioned by the irregular development of the corresponding part of the nervous system than to any peculiar deficiency in the process of ossification.

ROYAL IRISH ACADEMY.

December 12, 1836.—A paper was read "On the Seals of Ireland (Phocidæ)." By Robert Ball, Esq., M.R.I.A.

The author stated the circumstances by which he was led to discover that the seal of most frequent occurrence on the Irish coast was not defined as a British species, together with the subsequent identification of that animal, by Professor Nilsson, as the *Halichærus griscus* of his Scandinavian Fauna, (*Phoca Gryphus* of Fabricius,) found in the Baltic and North Sea. He asserted, however, that the habits of the Halichærus of this country differed so much from those ascribed to it in the Baltic, that it appeared to him not unlikely, on comparison, to prove a distinct species. He showed that the colour of the animal here varied so much from sex, age, season, &c., that it could not be considered of any value as a character of species in the present state of our knowledge of the subject. He alluded to the very small size of the brain compared with that of the genus Phoca, and stated that the intellectual powers bore the same proportion. Mr. Ball then proceeded to show that the simple form of the teeth of Halichærus (approaching closely to those of some species of Delphinus) furnished sufficient grounds for separating it from the genus Phoca; and observed, that the Halichærus may always be distinguished from other seals, by its straight profile, fierce aspect, and greater proportionate length. He mentioned the fact of his having discovered that the specimen in the British Museum, so long known as Donovan's *Phoca barbata*, (and the long-bodied seal of Parsons,) is formed of the skin of a Halichærus improperly stuffed; and he noticed the mistakes to which this has given origin.

Mr. Ball next gave instances of the occurrence in this country of the *Phoca Vitulina* (*P. variegata*, Nilss.), which he considered identical with the seal stated by Sir E. Home (Phil. Trans. 1822) to have been killed in the Orkneys, though it appears from the cranium

figured as if a few teeth of the *P. Groenlandica* were inserted into the upper jaw. The author related some anecdotes of the interesting and beautiful specimen now in the Zoological Gardens; contrasted the species in structure and habits with the Halichærus; and expressed his dissent from the statement put forward in Mr. Bell's British Quadrupeds, on the authority of Professor Nilsson, that the oblique position of the molar teeth in *P. Vitulina* was a specific character of unerring value. He has shown, in fact, that the obliquity in question arose from the insufficient development of the jaws in early life, which contracted the space for the teeth; and that it disappeared long before the skull reached its maximum size, and partially occurred in the young Halichærus.

Mr. Ball then alluded to the seal taken in the Severn, which Professor Nilsson pronounced to be his *Phoca annellata*, but which has since been stated, with the Professor's concurrence, to be the *P. Groenlandica*. He expressed his doubts as to the justness of this conclusion, observing that the *Groenlandica* was a large species, while the Severn seal was certainly a small one. He further showed that the form of the intermaxillary bones, where they joined the nasal, was quite sufficient to distinguish it from the specimen figured by Sir E. Home in the paper before referred to; and he expressed his belief that the species was still to be determined.

The author concluded by stating his belief in the existence of a fourth seal (probably *P. barbata*) on the southern coasts of Ireland, which he had occasionally seen, but never had opportunity of closely inspecting; and finally, exhibited a number of sketches illustrative of his paper, showing generic and specific distinctions of external forms, skulls, teeth, cæca, and of the great sinuses of the hepatic veins.

Professor Kane laid before the Academy specimens of the salts of a new acid, called by him "Xanthomethilic Acid."

The same gentleman stated some conclusions to which he had recently arrived, from the examination of pyroacetic spirit, which he considered to be a new alcohol.

LXXXIX. *Intelligence and Miscellaneous Articles.*

UPON THE SUPPOSED ABSORBENT POWERS OF THE CELLULAR POINTS, OR SPONGIOLES, OF THE ROOTS OF TREES, AND OTHER PLANTS. BY THOMAS ANDREW KNIGHT, ESQ., F.R.S. PRESIDENT OF THE HORTICULTURAL SOCIETY.*

AN opinion is very extensively if not generally entertained, that the nutriment which trees and other plants derive from the soil in which they grow, is exclusively taken in by the cellular extremities of their roots, which, from their texture, have been called Spongioles, and which in their organization differ from other parts of

* From the Transactions of the Horticultural Society, New Series, vol. ii. p. 117, having been read May 17, 1836.

the root in being totally without any alburnous or woody matter distinct from bark. But it is through the alburnum alone of trees, as I have proved by a great variety of experiments, and as is, I believe, generally admitted, that the ascending sap, under ordinary circumstances, passes up from the roots into their branches and leaves; and as this substance does not exist in the spongiole, my attention was directed to an inquiry, whether the spongioles possess the power of transmitting fluids, and if such power were found to exist in them, through what peculiar channels such fluids pass up: and as these questions are necessarily interesting, and to some extent, in particular cases, may become important to the practical gardener, I communicate the result of my experiments.

Spongioles are obtainable in the most perfect state from large seeds, such as those of the common or French bean, which have been permitted to germinate, by simply detaching them from the cotyledons, as they thus remain united to the caudex of the plant and its bud and plumule. Many of these were obtained from the seeds of plants of several kinds, and subjected to various modes of treatment in soils of different qualities, but all perished without a single plumule having expanded, or having apparently received any nutriment, either from the soil or other source. Yet the spongioles in these cases must have contained greatly more living organizable matter, derived from their cotyledons, than the whole body of the seed of a very large majority of plants can possibly contain; but they were, I conclude, incapable of transmitting it into the plumules owing to the want of alburnum.

I therefore believe my opinion, that spongioles are imperfectly organized parts of the plant, which neither absorb from the soil nor transmit fluids of any kind for the service of other parts of it, to be well founded; but alburnous matter is generated with great rapidity within them, and they become to a very great extent transmuted into perfect roots long before the growth of the stem or branches of the tree commences in the spring; and by these newly formed roots (but not by these exclusively) I conceive that nutriment is absorbed from the soil, and sent up into the leaves, to be there converted into the true sap of the plant. I am aware that the above-stated opinions are in opposition to those of many eminent physiologists, to which much deference is due, but I think that they have erroneously included within their spongioles portions of alburnous fibre, a substance never found in the organ properly called a spongiole.

ACTION OF PRESENCE.

The 60th volume of the *Annales de Chimie et de Physique* contains a paper by Pelouze on a new acid, which he calls *nitrosulphuric acid**. Speaking of the salt which this forms with ammonia, he says, "the excessive mobility of the elements of nitrosulphate of ammonia, and the stability which the alkalis impart to them, made me think it not

* A translation at length of M. Pelouze's paper has been given in Scientific Memoirs, part iii. p. 470.

impossible that this salt might exhibit phænomena of decomposition of the same class as the singular ones which M. Thenard observed with binoxide of hydrogen. In fact this is the case; many substances which decompose binoxide of hydrogen, without either acquiring or losing anything, also decompose the nitrosulphates. Spongy platina, oxide of silver, metallic silver, powdered charcoal, and oxide of manganese, produce this effect; the two first-mentioned especially act with extreme rapidity upon the nitrosulphate of ammonia.

"I convinced myself," continues M. Pelouze, "that this remarkable phænomenon was due, as in the case of binoxide of hydrogen, to *an action of presence* [à une action de présence], and that it never produces more than a mere conversion of nitrosulphate of ammonia into protoxide of azote and sulphate of ammonia. Oxide of silver is not reduced, for if it be washed after having caused it to decompose a large quantity of this salt, it afterwards dissolves in nitric acid without the disengagement of red vapours."

M. Pelouze afterwards remarks that "it would be very difficult to discover the probable cause of these singular phænomena; but the very circumstance of their being at present inexplicable, they appeared to me the more to merit the attention of chemists. What indeed is more calculated to excite curiosity than to observe a salt, by the mere contact of a body which neither yields anything to nor takes anything from it, decompose with extreme rapidity into new forms, among which the agent producing these violent actions remains chemically passive?

"Two compounds, namely, binoxide of hydrogen and hydruret of sulphur, are already known as possessing the property of decomposing by the influence of a simple action of presence."

Ann. de Ch. et de Phys., lx. 158.

CATALYSIS.—ACTION OF PRESENCE.

In the 61st volume of the *Ann. de Ch. et de Phys.*, Berzelius has followed up the remarks of Pelouze *on the action of presence*, and has given to it the name of *Catalysis*. His notice on this subject is entitled, "*Some ideas on a new force acting in the combinations of Organic Compounds.*"

In inorganic nature, new combinations are formed between the different bodies which are present, because these bodies have a greater tendency to combine with each other than with other bodies. Those bodies which have a great affinity for each other, combine together, and reject those for which they have a weaker affinity, and with which they were previously combined, and these also combine together. Until the year 1800, this tendency of bodies to unite was the only one known, except heat, and in some cases light, that could effect their combination. The influence of electricity was of later discovery, but it was soon also discovered the chemical and electrical affinity were the same, and that heat and light acted only by increasing or diminishing these affinities. On proceeding to the study of organic chemistry, very different bodies were obtained from the same crude material by different organs. In animals, this

crude material, which is the blood, flows in the uninterrupted vessels, and gives rise to all the different secretions; such as milk, bile, urine, &c., without the presence of any foreign body which could form new combinations.

Kirchoff discovered that starch dissolved in acids diluted with water was converted at a certain temperature into gum and then into sugar of grapes; and yet there was no combination between the elements of the acid and those of the starch, for no disengagement of gas took place. On treating the solution with bases, all the acid employed was regained; the solution contained only sugar, the weight of which only slightly exceeded that of the starch employed. Some time afterwards, Thenard discovered a new substance, the binoxide of hydrogen, the elements of which are held together by very slight affinity. This substance was not decomposed by acids, but alkalis gave its elements a tendency to separate; slow effervescence occurred with the disengagement of oxygen, and water was formed. It was soon found not only that soluble substances produced this effect, but also that other organic and inorganic bodies, such as manganese, silver, platina, gold, fibrin, &c., acted in the same manner upon it. This decomposition takes place by the mere presence of the foreign body, in consequence of a power which is at present unknown, and without the smallest quantity entering into the new compound, for the most minute researches failed in discovering the slightest alteration in it.

Edmund Davy discovered that if extremely finely divided platina be moistened with alcohol, this on taking fire ignites the platina, and the alcohol, if it contains water, is converted into acetic acid. This led to the grand discovery of Dœbereiner, that spongy platina has the property of firing a current of hydrogen directed upon it. This discovery was soon followed by that of Dulong and Thenard, who found that it was not platina alone that possessed this property, but that other bodies, such as gold, silver, and glass, acted in the same way, but only when they are exposed to a somewhat high temperature; whereas platina, iridium, and other metals which accompany platina, produce this effect even much below the temperature of melting ice.

The analogy was observed which exists between the phænomena of the conversion of sugar into alcohol by the presence of a foreign insoluble body, and that of the decomposition of peroxide of hydrogen into water and oxygen, by the presence of platina, silver, fibrin, and some other equally insoluble bodies. No case analogous to that of the decomposition of binoxide of hydrogen by the presence of the alkalis dissolved in this substance was known, for at this period the analogy of this phænomenon with that of the formation of sugar by means of starch and sulphuric acid was not known. Something similar occurs in one of the hypotheses on the formation of æther. According to this hypothesis, sulphuric acid should combine with one part of the water contained in the alcohol, and thus would form æther; but it could not be explained why other bodies, such as potash, chloride of calcium, quicklime, &c., which have a

very great affinity for water, did not produce the same effect. Mitscherlich showed that if alcohol be poured upon sulphuric acid, at a temperature higher than that of boiling water, water and æther are distilled together and form a mixture, the weight of which is precisely equal to that of the alcohol employed. Thus the sulphuric does not there act in consequence of its affinity for water; its action is analogous to that of the alkalis on the binoxide of hydrogen; and that of sulphuric acid upon starch in the formation of sugar.

It is then proved that many bodies, both simple and compound, soluble and insoluble, have the property of exerting on other bodies an action which is very different from chemical affinity. By means of this action they produce decompositions in bodies, and form new compounds into the composition of which they do not enter.

This new power, hitherto unknown, is common, both in organic and inorganic nature. I do not believe that it is a power which is entirely independent of the electro-chemical affinities of the substance; I believe, on the contrary, that it is merely a new form of it; but as long as we do not see their connection and mutual dependence, it will be more convenient to describe it by a separate name. I shall therefore call it *catalytic power*. I shall also call *Catalysis* the decomposition of bodies by this force, in the same way as the decomposition of bodies by chemical affinity is termed analysis.

The following are the questions which occur relative to this catalytic power:

Can this catalytic power produce differences in catalytic products, according to its greater or less intensity?

Can different bodies possessing catalytic power produce different catalytic effects upon the same compound substance?

Can bodies which possess catalytic power exert this action on a great number of different compound substances, or is this action confined to a few substances?

These questions can be answered only by extended researches; it is enough at present to have established the existence of this power by a sufficient number of examples. This power gives rise to numerous applications in organic nature; thus it is only around the eyes of the potato that diastase exists; it is by means of catalytic power that diastase, and that starch which is insoluble, is converted into sugar and into jam, which being soluble form the sap that rises in the germs of the potato. This evident example of the action of catalytic power in an organic secretion is not probably the only one in the animal and vegetable kingdom, and it may hereafter be discovered that it is by an action analogous to that of catalytic power that the secretion of such different bodies is produced, all which are supplied by the same matter,—the sap in plants and the blood in animals.

M. LEVEILLE ON THE HYMENIUM OF FUNGI.

M. Léveillé lately presented to the Academy of Sciences of Paris

a paper on the *hymenium*, or fructifying membrane, of the Fungi. In this paper the author endeavours to establish that the *Hymenothecii* which form the fifth order of Persoon's *Synopsis Fungorum*, which all authors have adopted and which composes the first class of Fries's *Systema Mycologicum*, are too numerous; that they contain genera which have no relation to one another; and that they ought to be divided into two classes.

M. Léveillé has taken as a basis the structure of the *hymenium*; he says, that if the surface of the lamina of *Agaricus micaceus* is examined in profile with a microscope, two kinds of organs are seen, the first vesicular, projecting, diaphanous, conical, or cylindrical, in form of a club, and placed at certain distances one from the other; the others representing mammæ, more or less projecting, very close to one another, and ending in four points, each of which bears a spore. The author gives to the first of these organs the name of *cystides* (from *Cystidia*), to the second that of *barides* (from *Baridia*). The *cystides* do not exist in a great number of species, but the *barides* may always be found; their existence may be shown on the first Agaric met with, on the *Dædaleæ*, *Boleti*, *Hydna*, *Thelephoræ*, and *Clavariæ*; they are tetrasporous, disporous, or monosporous, according as they have two or four divisions; or are simple, as in the *Tremellæ*. These organs are not new; Micheli made them known, as also did Bulliard and Nees von Esenbeck.

After some general remarks on these organs, on the spores, and their mode of emission, M. Léveillé shows that modern authors have erroneously taken for cells filled with spores the hymenial tissue itself, which is composed of elongated cells, most of them having the same direction, *parallel* to the plane of the receptacle upon which they are fixed, and not *perpendicular* as M. Nees has represented them in his *System der Pilze*.

The author then treats of the structure of the *hymenium* of the Helvelloïdes, which Hedwig made known. Taking as an example *Peziza aurantia*, and examining a slice with the microscope, he found that this *hymenium* is composed of elongated cells placed side by side, perpendicular to the plane on which they are fixed; their structure consists in a single membrane, thin and diaphanous; they contain eight spores, which they eject like a cloud, suddenly, and from time to time in the air. Among the cells others of the same length are seen; they are diaphanous, empty, and filiform. Hedwig calls the first *thecæ*, the second *paraphyses*; the existence of the latter, like that of the cystides, is not constant. A plate, drawn after nature, by M. Decuisne, represents all these organs taken from several species of Fungi, and shows that the terms free or fixed utricules, *asci fixi*, *asci liberi*, employed by authors to distinguish the fructification of *Agarici*, *Boleti*, *Clavariæ*, &c., from those of the *Helvellæ*, *Pezizæ*, or of the *Geoglossa*, are improper, and not in the least suitable. Their differences being known, the division of Persoon's fifth order or of Fries's first class into two classes is natural. The first takes in the *Hymenomycetes*, or *Basichospori*; the second the *Hymenothecii*, or *Thecospori*. The one is composed of *Agarici*,

Boleti, Hydna, Clavariæ, Thelephoræ, Pistillariæ; the other of the *Helvellæ, Morellæ, Pezizæ, Spathulariæ, Geoglossa*, &c. The characters of these two classes have been developed enough in the foregoing, and it will not be necessary to repeat them.

By making an application of this new division, the genera *Sclerophyllum* and *Craterellus* will be seen to belong to the first class by their spores; the *Asterophora* of Dittmar belongs to the Birsoïdeæ, and determines by its presence the abortion of a species of *Agaricus*. M. Léveillé has often found perfectly distinct organs of fructification on its *hymenium*; the genus *Solenia* and the *Peziza perula* of Persoon must also be referred to this class. It will be necessary to bring back to the *Thecospori* the genera *Spathularia, Geoglossum, Mitrula*, and perhaps *Sparassis*, which have been placed by the side of the *Clavariæ* more on account of their form than their fructification. Finally, the genera *Sclerotium, Acrospermum, Sclereglossum*, and *Spermœdia*, whose fructification is not yet known, must be separated from both the classes. The author thinks that the *Cantharellus Dutrochetii*, which, according to the drawings of M. Turpin, presents the fructification of the *Lycoperdaceæ* or of the *Sporotrichum*, if examined again would come into the first class.

He concludes by observing that he adduces a proof in favour of the opinion which M. Turpin has started in the *Mémoires du Muséum d'Histoire Naturelle*, and which consists in regarding the number two as the multiple of the Acotyledons, three and five those of the Monocotyledons and Dicotyledons. *L'Institut*, No. 203.

ASCENT OF THE SAP.

M. Dutrochet communicated the following observations on the ascent of the sap to the Academy of Sciences of Paris. When a branch with leaves is immersed with its basis into water, we know, by the experiments of Hales and Sennebier, that light exercises a great influence on the ascent of the sap from the attraction of the leaves, and that this ascension diminishes considerably in the dark. Sennebier remarked that this influence varied in different species of plants, but he deduced from his experiments no general fact. M. Dutrochet thinks he is able to confirm the following, viz. that those plants which attract the most water in light are those which in the dark attract the least; and, on the contrary, those which in the light attract least, attract the most in the dark.

L'Institut, No. 203.

MAGNETIC OBSERVATIONS OF THE AURORA OF FEB. 18TH.

The following article may supply the deficiency of magnetic observations regretted by Mr. Heineken at p. 266 of our present volume.

A letter from M. Matteucci, dated from Forli, in the states of the Church, to M. Donné, announces that the Aurora Borealis of the 18th of Feb. exhibited in that neighbourhood a magnificent spectacle in the north and west of the heavens, from nine in the evening

till one in the morning. This letter contains the observations made by M. Matteucci, while this phænomenon lasted, on two magnetic needles, the one of the length of $0^{m}\cdot05$; the other shorter, which in ordinary had 32 and 16 oscillations per minute. In ten successive observations made during the aurora borealis, the constant number of oscillations amounted to 30 and 13 only. The next morning, the temperature remaining the same as in the preceding evening, the needles had returned to their ordinary rate of 32 and 16 oscillations. M. Matteucci adds, that if similar facts should be observed hereafter, they might serve to elucidate the theory of the aurora borealis and of terrestrial magnetism.—*Institut*, March 22.

Observations of this phænomenon are also given in the Proceedings of the Astronomical Society for March 1837.

In London, by F. Baily, Esq., the President of the Society.—"The atmosphere had been very cloudy, with much rain and wind during the whole of the evening. But a little after ten o'clock (mean time), or about three quarters of an hour before the occultation of Mars by the moon took place, the clouds dispersed and left a beautifully clear sky; at the same time discovering a most brilliant *aurora*, or rather stream of light, which extended from the horizon in the west, through the zenith, almost down to the horizon in the east. This light was of a fine rose colour, being most vivid and brilliant in the western horizon; and, as it approached the zenith, was evidently impaired by the strong light of the moon, then near her full, and high on the meridian. The *aurora* appeared very steady, and unaccompanied by any coruscations. It lasted till about half-past eleven o'clock, and wholly disappeared before midnight. By subsequent accounts which have been received, it was seen in the western parts of Ireland, and in Scotland."

At Makerstoun, near Kelso, Scotland, lat. 55° 34′ 45″ N., long. 10^{m} 4^{s} west of Greenwich; by Sir T. M. Brisbane.—"On quitting the observatory, at about 10^{h} 35^{m}, I was struck by a most extraordinary red appearance in the sky, extending as far west as the constellation of Orion, and as far east as that of Leo. It was principally north of the moon—none of it very near her; and might have been at least 10 degrees in breadth, of almost a deep red colour. I found all the servants out, looking at it with surprise and astonishment. It was followed next day by snow, rain, and a gale of wind. Observed also a very beautiful lunar halo, of nearly 5′ in diameter: the colour principally orange, but it did not last many minutes."

At Ashurst, in Kent, by Mr. Snow.—"A very remarkable Aurora Borealis became visible at ten o'clock. It commenced with a very fine deep red arch, extending over the zenith from the east and west, and slanting away, as it were, at an altitude of about 30° towards the horizon in the N.E. and N.W. Its appearance was magnificent, although the moon was very bright, and not far from the meridian. When it was first noticed, the clouds that were driving off towards the east were deeply tinged with the red colour. After

undergoing some changes, difficult to describe, it grew fainter at twenty minutes before eleven. A white arch then appeared in the usual situation of the Aurora, giving out white streamers at a few minutes before eleven. This latter arch was not well defined. At ten minutes after eleven the Aurora had almost entirely disappeared, excepting a reddish patch between Arcturus and Regulus, which in ten minutes more was no longer to be seen. After this the wind, which during the day had been at S.W., shifted to the N.W., with a cloudless sky, and remained so until the morning. The stars, however, became gradually very ill defined; and during the whole of the next day and night there fell unceasing torrents of rain, with a gale of wind from the south-west."

ON THE DECOMPOSITION OF CARBONATE OF LIME BY HEAT.

M. Gay-Lussac observes, that it has long been supposed that the calcination of limestone is accelerated by the presence of water; and the opinion appears to be adopted by lime-burners in general. M. Dumas admits the influence of water to be unquestionable, and he gives two explanations of its action; either, says he, it acts upon the carbonate, and forms a temporary hydrate, taking the place of the carbonic acid for a very short time, for the hydrate of lime itself is decomposed by a red heat; or the water being decomposed by the carbon, employed as a combustible, is converted into various gases, of which carburetted hydrogen forms a part, and this reacting upon the carbonic acid of the carbonate tends to convert it into oxide of carbon, and thus facilitates the separation from the carbonate of lime. Thus, limestone fresh quarried, and consequently still moist, ought to be more readily calcined than the stone which is nearly dry; and most lime-burners are well acquainted with this fact, and sprinkle with water the limestone which has been long procured before they charge the kilns with it. (Dumas, *Traité de Chimie*, ii. p. 482.)

The first of these explanations is, however, inadmissible, for hydrate of lime is decomposed at a temperature considerably lower than that at which carbonate of lime is decomposed under the influence of water.

On considering the circumstances of the combustion in limekilns, the second explanation does not appear to M. Gay-Lussac to be applicable, and he therefore proceeds to some observations which he thinks will explain the influence of the water.

A porcelain tube was filled with bits of marble and placed in a furnace, the heat of which was easily regulated; a glass retort containing water was adapted to one end of the tube, and at the other end a glass tube to receive the carbonic acid gas. The heat was raised sufficiently high to decompose the marble, and on shutting the ash-pit door the heat fell to a dull red, and the carbonic acid ceased to come over; and at this instant the water was boiled in the retort, and carbonic acid was abundantly obtained. On discontinuing the vapour, the disengagement of acid instantly ceased, and returned only on continuing the vapour.

It appears, therefore, proved that the vapour of water actually favours the decomposition of the carbonate of lime by heat, and that by its operation this decomposition may occur at a lower temperature than is otherwise requisite.

M. Gay-Lussac considers the action of the water to be entirely mechanical. When the carbonate of lime is exposed to heat, and is near the point of decomposition, an atmosphere of carbonic acid is formed around it, which presses upon the acid remaining combined, so that the latter, that it may be disengaged, must overcome the pressure of this atmosphere. This, however, cannot occur without still further raising the temperature, or removing the carbonic acid and forming a vacuum, or by displacing it, either by the vapour of water, or some other elastic fluid, such as atmospheric air.

This explanation is supported by the following experiment; carbonate of lime was heated in a porcelain tube nearly to its decomposing point, and then a current of atmospheric air was passed over it. The disengagement of carbonic acid immediately commenced, continued with the current of air, ceased when it was stopped, and recommenced with it.

M. Gay-Lussac, therefore, considers it as proved, that the influence of aqueous vapour, in the calcination of limestone, is confined to the production of a vacuum for carbonic acid, and to the prevention of the pressure of the disengaged acid upon that which remains with the lime. When the vapour is present, a lower temperature is required to dislodge the carbonic acid; but the importance of this influence must not be overrated. The water, in calcareous stones, is mechanically interposed between their particles; and with the exception of some minute portions, which remain confined in the centre of pieces too large to allow of the heat penetrating and vaporizing them, the greater part of the water must evaporate without any useful result; and, on the contrary, with the loss of fuel, before the limestone has acquired the temperature requisite for its decomposition.

M. Gay-Lussac thus concludes: "I am certainly convinced that the vapour of water favours the calcination of limestone, but I am doubtful as to its possessing real advantages, because there is not a great difference in the temperature at which it decomposes, alone or with the vapour of water: besides, if the vapour of water only exerts a mechanical action similar to that of atmospheric air, any important advantage which it possesses over the aeriform current continually passing over the burning mass, is not evident.

The readier decomposition of carbonate of lime by the access of aqueous vapour, or rather by means of a vacuum, cannot be considered as an isolated fact. It may be regarded as an established principle, that when one or several gaseous products are obtained, either by the action of heat or a chemical agent, the decomposition may be generally facilitated, by keeping the bodies in vacuo, or by preventing the gaseous fluids from pressing upon it. And on the other hand, the decomposition may be retarded or entirely pre-

vented by forming round the body a sufficient pressure of an elastic fluid of the same nature as that which is to be disengaged. It is thus in the curious experiment of Hall; carbonate of lime was fused at a very high temperature, without being decomposed, under the influence of a sufficient pressure of carbonic acid.

Ann. de Ch. et de Ph. lxiii. 219.

PREPARATION OF IODINE. BY M. BUSSY.

The process of extracting iodine generally followed, and which consists in decomposing the mother-waters of kelp by means of concentrated sulphuric acid, is well known to be liable to variable results, on account of a portion of the iodine coming over in the state of hydriodic acid and chloride of iodine, so that in either case there is always a considerable loss.

To avoid this inconvenience, M. Soubeiran proposed to precipitate the iodine from the mother-waters by means of sulphate of copper, and afterwards to decompose the iodide of copper by peroxide of manganese at a high temperature*. But this process requires minute attention and much precaution to separate the whole of the iodine existing in the mother-waters, and we do not believe that it has ever been employed in any manufactory.

These circumstances induce me to publish a far more simple method, which has been lately employed by some manufacturers of iodine; it was discovered (if we are rightly informed) by M. Barruel, director of the chemical works at the Faculty of Medicine; it consists in precipitating the iodine of the mother-waters of kelp by a current of chlorine.

In this process the mother-waters are first to be evaporated to dryness, and to the residue there is to be added a tenth of its weight of powdered peroxide of manganese; the two substances are to be perfectly mixed, and subjected to a dull red heat in an iron vessel, frequently stirring them; the object of this calcination is to convert the sulphurets and hyposulphates, of which the mother-waters contain a large quantity, into sulphates. It is extremely easy to determine when these compounds are converted into sulphates by taking a small quantity of the calcined matter and pouring upon it an excess of sulphuric acid. It ought not to yield, when the conversion is perfect, either sulphuretted hydrogen or a deposit of sulphur. If violet vapours are disengaged during the calcination, the heat must be lowered to avoid loss. When the sulphurets are decomposed, the residue is to be dissolved in a sufficient quantity of water to give a solution of sp. gr. 1·333; through this solution there is then to be passed a current of chlorine gas, taking care to stir it constantly with a glass rod: the liquor becomes of a deep brown colour, afterwards turbid, and deposits iodine in the form of a black powder; it is to be collected and distilled in a glass retort, in order to procure it in crystals. The only difficulty in this process is to determine the point at which the action of the chlorine should cease, that no

* *Journ. de Pharmacie,* tom. ii. p. 411.

excess may be used which would react upon the iodine precipitated. This excess of chlorine is especially to be apprehended, when it is also intended to separate from the same mother-waters the bromine which they contain.

In order to avoid adding excess of chlorine, the liquor which is supposed to be near saturation, may be suffered to settle for a short time, and the current of chlorine being interrupted it is to be directed on the surface of the liquid. If it contain any iodide in solution, a pellicle of iodide will be observed to form on the surface; this effect is not produced when all the iodine is precipitated; in the latter case the liquor becomes quickly clear and retains only a reddish tint.

PREPARATION OF BROMINE. BY M. BUSSY.

The separation of bromine, as usually performed, also involves great difficulties, which may be avoided by the following process.

This process greatly resembles the preceding one; it is founded, like it, upon the great affinity of chlorine and the property which it possesses of displacing bromine from its combinations. It also includes the employment of the mother-waters of iodine, which without it are useless. Take the mother-waters of kelp, after having separated the iodine by chlorine as above described. These mother-waters contain a metallic bromide, when care has been taken to avoid the use of more chlorine than sufficient to precipitate all the iodine. Add to 1250 parts of these mother-waters, thirty-two parts of powdered peroxide of manganese, and twenty-four parts of sulphuric acid of sp. gr. 1·848: the whole is to be put into a tubulated glass retort, to which a tubulated receiver is to be adapted, and to this a tube which is immersed in a test tube. The retort and the receiver, as well as the receiver and the tube, ought to fit sufficiently well without the use of lute or corks, as they would be inevitably destroyed by the action of the chlorine. All being thus arranged, the retort is to be heated, so as to boil the liquid; the bromine condenses in the receiver in the form of red oily striæ, with a small quantity of water; the operation is to be discontinued when red vapours cease to be produced. On heating the receiver gently, without dismounting the apparatus, the bromine will pass into the test-tube, where it will condense on cooling.

The mother-waters which have been made use of may be rejected, on ascertaining by the addition of a fresh quantity of sulphuric acid and peroxide of manganese that they contain no more bromine. *Journal de Pharmacie*, January 1837.

ON THE RED AND WHITE OXIDE OF PHOSPHORUS. BY M. MULDER.

Pelouze considers the white crust which covers old sticks of phosphorus as hydrate, and H. Rose regards it as differing from common phosphorus only by its state of aggregation. The red powder which sometimes covers phosphorus may be prepared by

passing a current of oxygen gas on phosphorus melted under hot water, in which operation the phosphorus combines with this gas, giving out light and heat. It is therefore proved that the red powder is an oxide.

Some time since I received some sticks of phosphorus which had been kept for thirty years in the same bottle and exposed to the light. Their surface was perfectly white, and covered with a crust of this white matter to the thickness of about $\frac{4}{100}$ of an inch. I removed these cylinders into another bottle filled with pure distilled water, and was surprised on finding the day after that they had become entirely red: although they have been four years in contact with light, they retain the same fine red colour.

This observation is adverse to the opinions of Pelouze and Rose. The only idea which I can form as to this sudden change of the white crust into red oxide by distilled water is, that the minute quantity of oxygen in the distilled water had converted the white crust into oxide, and since this quantity of oxygen is very small, the white matter could not be an oxide. I imagined that the phosphuretted hydrogen, which is always formed by phosphorus in water, might be the cause of it.

To satisfy myself respecting it, I passed a current of phosphuretted hydrogen gas into water containing red oxide of phosphorus in a state of minute division. By this operation the red oxide was gradually reduced to the white matter; which in its turn was in some days after changed into red oxide, on removing it, without the contact of light, into fresh water, in which oxygen was dissolved.

It is evident from this that the white crust of phosphorus is a combination of phosphuretted hydrogen and red oxide of phosphorus, and that there is therefore only one oxide of phosphorus, and which is of a red colour.

On keeping phosphorus in water the latter is decomposed; oxide of phosphorus and phosphuretted hydrogen are formed, a part of which combines with the oxide of phosphorus, and gives rise to the white matter described. *Journal de Pharmacie*, Jan. 1837.

ACTION OF IODINE ON THE VEGETABLE ALKALIS.

M. Pelletier remarks that it has not been determined whether the bodies which are termed *halogen* combine with the vegetable alkalis, or exert an elementary action upon them which alters their composition. Are iodine, bromine, and chlorine converted into iodides and iodates, bromides and bromates, chlorides and chlorates by the influence of the salifiable bases? Do iodites, bromites, and chlorites exist, or is the organic base decomposed; and if so, is chlorine, bromine, or iodine substituted for hydrogen? These are the principal points which attracted M. Pelletier's attention, and he arrived at the conclusion that iodine is capable of combining immediately with the greater number of organic salifiable bases; and that from its union with these substances there result definite combinations of this elementary body and the vegetable alkalis.

Iodine and Strychnia.—The properties and composition of this alkali, its insolubility, and the facility with which it forms definite compounds and crystallizable salts, were the reasons for selecting it in preference to others in commencing the investigation. Strychnia triturated with half its weight of iodine became of a reddish brown colour; distilled water was added and the trituration continued. The filtered solution was colourless, and neither acid nor alkaline, and exhibited but slight traces of iodine or strychnia. The matter remaining on the filter was treated with boiling water, which became of a light rose colour; when filtered and evaporated it left a very light residue. It consisted of iodide with a small portion of hydriodate of strychnia, as was afterwards proved.

The brown matter insoluble in water was subjected to the action of boiling alcohol; it dissolved entirely, and the solution was of an orange yellow colour: by cooling it deposited a great quantity of small laminated crystals, of an orange yellow colour and resembling aurum musivum. More and similar crystals were obtained by evaporation; but quite at the end of the operation white acicular crystals were procured which were hydriodate of strychnia; both these kinds of crystals may be obtained by the direct action of alcohol on a mixture of iodine and strychnia.

This laminated substance has the following properties: it is insoluble in cold water, and very slightly soluble in boiling water; it is but little dissolved by weak alcohol, its best solvent is strong boiling alcohol; on cooling the greater part of it separates in micaceous scales; it is insoluble in sulphuric æther.

Its taste is at first but slight, but after a certain time it is bitter and slightly astringent. It is infusible at 212°, and at all temperatures below its point of decomposition; when heated upon platina foil, it softens, swells, yields iodine, and chars almost at the same time, emitting the smell peculiar to organic salifiable bases when decomposed by heat.

Acids, according to their nature and concentration, act differently on this substance. In general when they are very dilute and cold, they do not act sensibly upon it; but by long ebullition they separate the iodine and unite with the strychnia, which may be precipitated by ammonia.

Concentrated nitric acid, even when cold, separates iodine and destroys or alters the organic matter; concentrated sulphuric acid produces the same effect, but not so rapidly. Concentrated hydrochloric acid has no effect upon this substance when cold, but when heated it combines with the strychnia and separates the iodine.

Ammonia does not act upon this substance, either cold or hot; and potash and soda affect it only when heated; a little strychnia is separated, and iodide of potassium or sodium remains in solution. By the successive action of acids and alkalis upon the micaceous compound, it is eventually decomposed, and they show that it is an iodide of strychnia.

The proportions of the iodine and strychnia, which exist in com-

bination, were determined by the action of nitrate of silver upon it. Iodide of silver is precipitated, and the strychnia, which has suffered some change, combines with the nitric acid.

The analysis was performed by determining the quantity of iodide of silver obtained, for the iodine; and another portion of the compound, deprived of its iodine by the action of acids and alkalis, was decomposed by oxide of copper. The salt consists of

One equivalent of iodine	126
One equivalent of strychnia	234—360

Iodate of Strychnia.—Supposing the iodide of strychnia to be converted into iodate of strychnia, it would consist of

One equivalent of iodic acid....	166....	41·5
One equivalent of strychnia	234....	58·5
	400	100·

In order to compare the formula indicated by theory with the composition of the salt yielded by direct analysis, strychnia reduced to fine powder was diffused through warm water, and iodic acid was added to it, dissolved in rather a large quantity of water; by filtration and evaporation flat acicular crystals were obtained. Care must be taken to avoid excess of acid, as by its action upon the strychnia an acidulous iodate is obtained of a red colour. Iodate of strychnia may also be procured by decomposing neutral sulphate of strychnia by means of iodate of barytes. The crystals thus obtained resemble cyanide of mercury in appearance.

The following method was employed for analysing this iodate and the organic iodates in general; the iodate of strychnia was dissolved in water and decomposed by excess of potash. The liquor without filtration was evaporated in the capsule which contained it; iodate of potash remained, with free strychnia and excess of potash; the strychnia was decomposed by calcination, adding a little nitrate of potash in order to burn the carbon; the iodide of potassium was redissolved in the water and precipitated by nitrate of silver; the iodide of silver is to be treated with dilute nitric acid to separate the oxide of silver; care must be taken not to saturate the solution with nitric acid before precipitation.

To determine the quantity of strychnia the salt was heated with oxide of copper, and calculating from the usual data, the composition of the iodate of strychnia appeared to be

Iodic acid.................	41·64
Strychnia	58·36—100

These proportions agree almost precisely with those already deduced from theory; and show that it is a neutral salt consisting of an equivalent each of acid and base.

Hydriodate of Strychnia.—The neutral salt could not be obtained: the subsalt procured is but slightly soluble; it may be formed either

by directly combining the acid with the alkali, or by decomposing a soluble salt of strychnia by an alkaline hydriodate. This salt is white, formed of small laminæ or flattened needles adhering together; although but slightly soluble in cold water, its taste is very bitter; it is more soluble in alcohol than in water. It does not act upon litmus paper. It is a subsesquihydriodate, composed of

One equivalent of hydriodic acid	127 ..	26·6
One and a half equivalent of strychnia	351 ..	73·4
	478	100·

The following singular reaction, in the opinion of M. Pelletier, throws great light on the theory of organic iodides: When a solution of a neutral iodate of strychnia is poured into a solution of an hydriodate of the same base, no apparent decomposition takes place; but if iodic acid or an acidulous iodate be substituted for a neutral one, a brown precipitate is obtained, formed of iodide of strychnia and free iodine. When this precipitate is macerated in a solution of bicarbonate of potash, the excess of iodine dissolves, and the iodine then assumes the orange yellow colour which belongs to it, and it then resists the action of the bicarbonate.

In this decomposition the five equivalents hydrogen of the hydriodic acid of five equivalents of hydriodate of strychnia combine with five equivalents of oxygen of the iodic acid to form water, and the five equivalents of strychnia are precipitated with six equivalents of iodine, one of which is dissolved by the bicarbonate of potash, and five equivalents of neutral iodide of strychnia remain.

If also an acid be added to a mixture of neutral iodate of strychnia and of hydriodate, either neutral or a subsesquihydriodate, a brown precipitate is formed of iodine and iodide of strychnia. This action is explained by what is above stated.

An. de Ch. et de Phys. tom. lxiii. p. 164.

METEOROLOGICAL OBSERVATIONS FOR APRIL 1837.

Chiswick. —April 1, 2. Fine. 3—6. Cold and dry. 7. Hail showers. 8—10. Bleak and cold, with an exceedingly dry atmosphere. 11. Clear and fine. 12. Hazy: bleak and cold. 13. Overcast: cold and windy. 14. Overcast. 15. Clear: fine. 16. Snowing: cloudy and cold. 17. Cloudy and cold. 18. Overcast: slight rain. 19. Hazy: fine. 20. Fine: rain. 21. Rain. 22. Fine. 23. Rain. 24. Very fine. 25. Fine. 26. Very fine. 27. Slight rain: fine. 28, 29. Slight rain. 30. Rain: fine.

Boston.—April 1, 2. Fine. 3. Cloudy: rain and snow early A.M. 4. Fine; snow early A.M. 5. Fine. 6. Fine: hail early A.M. 7, 8. Fine. 9. Snow: snow P.M. 10. Fine. 11, 12. Cloudy. 13. Snow. 14. Fine. 15. Cloudy. 16. Rain and snow. 17. Stormy. 18—20. Cloudy. 21. Fine: rain P.M. 22. Cloudy: rain early A.M.: rain and hail P.M. 23. Cloudy: rain A.M. 24. Cloudy. 25. Fine. 26. Fine: rain early A.M. 27. Cloudy: rain P.M. 28. Fine. 29. Cloudy: rain A.M. and P.M. 30. Cloudy.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary; by Mr. THOMPSON at the Gardens of the Horticultural Society at Chiswick, near London; and by Mr. VEALL at Boston.

Days of Month. 1837. April.	Barometer. London: Roy. Soc. 9 A.M.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ A.M.	Thermometer. London: Roy. Soc. Fahr. 9 A.M.	Thermometer. London: Roy. Soc. Self-registering. Min.	Thermometer. London: Roy. Soc. Self-registering. Max.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8½ A.M.	Wind. London: Roy. Soc. 9 A.M.	Wind. Chisw. 1 P.M.	Wind. Bost.	Rain. London: Roy. Soc. 9 A.M.	Rain. Chisw.	Rain. Boston.	Dew-point. Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
S. 1.	29·891	29·964	29·916	29·50	42·0	34·5	44·2	51	25	37	SW.	NW.	calm	...	...	...	47
⊙ 2.	29·893	29·956	29·819	29·55	37·7	32·0	48·3	50	35	38	WSW.	N.	calm	...	·03	...	32
M. 3.	29·448	29·771	29·509	28·95	43·4	37·7	46·0	49	30	38	SW.	W.	W.	...	...	·09	35
T. 4.	29·613	29·725	29·636	29·21	38·4	33·6	47·0	44	34	37	SW.	NE.	NW.	...	...	...	31
● W. 5.	29·662	29·741	29·720	29·46	38·6	35·2	44·0	44	31	38·5	NE. var.	NE.	E.	...	...	...	29
Th. 6.	29·869	30·013	29·936	29·54	39·3	33·0	43·8	48	29	37	N.	N.	N.	...	...	...	32
F. 7.	30·063	30·310	30·122	29·70	39·7	32·7	45·4	47	33	37	N.	NE.	N.	...	·02	...	33
S. 8.	30·306	30·376	30·356	30·02	39·7	34·9	43·6	42	27	38	N.	NE.	N.	...	...	...	31
⊙ 9.	30·263	30·332	30·242	29·98	35·2	30·5	44·3	42	26	36	NNW.	NE.	E.	...	...	...	23
M. 10.	30·063	30·156	29·892	29·78	35·7	29·0	40·2	43	23	38	NNE.	NE.	E.	...	...	...	25
T. 11.	29·637	29·720	29·646	29·34	33·7	29·4	39·9	45	24	34	W.	S.	calm	...	...	...	26
☽ W. 12.	29·623	29·788	29·680	29·39	35·7	28·0	39·8	42	34	37·5	E.	E.	calm	...	·01	...	28
Th. 13.	29·830	29·938	29·889	29·57	36·9	35·5	40·3	40	34	37	NE. var.	NE.	E.	...	...	...	29
F. 14.	29·865	29·937	29·849	29·57	38·8	35·2	40·4	48	25	40	N.	NE.	E.	...	...	...	32
S. 15.	29·629	29·719	29·493	29·27	40·2	34·5	45·0	55	33	42	S.	E.	calm	...	·01	...	32
⊙ 16.	29·352	29·523	29·424	29·00	37·2	35·4	47·7	41	34	37	W.	NE.	calm	...	·01	·36	32
M. 17.	29·640	29·831	29·762	29·30	37·7	35·2	39·8	50	37	35	W. var.	N.	NE.	...	...	·14	30
T. 18.	29·857	29·980	29·927	29·50	40·3	37·4	48·0	43	41	41	N.	N.	calm	...	·01	...	35
W. 19.	29·916	29·978	29·914	29·50	43·8	40·2	44·5	60	29	43·5	SW.	N.	calm	...	...	...	37
○ Th. 20.	29·762	29·840	29·809	29·30	49·9	49·8	50·7	56	28	43	SW.	W.	calm	...	·01	...	39
F. 21.	29·699	29·810	29·665	29·26	46·2	36·5	56·2	46	32	46	SSE.	S.	W.	·069	·20	...	37
S. 22.	29·501	29·639	29·560	29·16	43·7	39·3	47·2	51	39	45	WSW.	W.	calm	·144	·04	·22	38
⊙ 23.	29·608	29·673	29·657	29·11	44·3	39·8	48·0	53	35	40	SW.	W.	calm	·101	·06	·72	39
M. 24.	29·667	29·842	29·734	29·30	48·3	41·2	49·0	60	30	44	S.	SW.	NW.	·069	·24	·07	40
T. 25.	29·853	29·932	29·880	29·43	49·6	40·5	55·5	58	44	51	SSW.	SW.	calm	...	·21	...	43
W. 26.	29·784	29·866	29·840	29·33	52·4	47·2	53·8	64	38	53	SW.	SW.	calm	·250	·02	·20	46
☾ T. 27.	29·718	29·778	29·670	29·24	48·5	43·2	59·2	54	33	49·5	S.	S.	calm	...	·04	...	45
F. 28.	29·626	29·700	29·550	29·20	51·6	42·2	52·5	55	42	51	SSW.	S.	calm	·116	·01	·08	45
S. 29.	29·459	29·552	29·376	29·07	49·3	46·3	52·4	53	47	48	SSE.	S.	calm	...	·19	...	46
⊙ 30.	29·401	29·641	29·763	28·85	52·8	49·2	53·4	61	46	55	SSW.	S.	calm	·151	·02	·12	48
	29·750	29·868	29·868	29·37	42·4	37·3	47·0	49·83	33·26	41·5				Sum ·900	1·13	2·00	35·5

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END OF THE TENTH VOLUME.

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